

# The American Historical Review

THE AMERICAN HISTORICAL ASSOCIATION



## THE APPARITION AT KNOCK CO MAYO,

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MANY MIRACULOUS CURES HAVE BEEN EFFECTED THERE SINCE THE ABOVE OCCURRENCE.

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# The American Historical Review

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## The Devotional Revolution in Ireland, 1850–75

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EMMET LARKIN

"IF YOU KNEW," a Waterford priest wrote Tobias Kirby, the new rector of the Irish College in Rome, on January 3, 1850, "all there is to remedy, all the evil there is to check!"<sup>1</sup> "We have not had," he further explained to Kirby, referring to the decline in clerical discipline after the famine, "a Conference here since the beginning of the distress, four years now probably—& but *one* retreat all that time & everyone doing & thinking & speaking as it listeth him, & no one to prevent it." The occasion for this lament was the recent and encouraging news from Rome that Paul Cullen, Kirby's predecessor as rector of the Irish College, had just been appointed archbishop of Armagh and the accompanying rumor that the new primate had also been armed with the power of apostolic delegate by Pius IX and instructed to summon a national synod for the better government and regulation of the Irish Church. More than a quarter of a century after Cullen's arrival in Ireland, his cousin and protégé, Patrick Francis Moran, the bishop of Ossory, was able to report to Kirby in a matter-of-fact way from Kilkenny during the course of a letter that "we ended two small Missions in two of our city Churches on Sunday last, preparatory for Christmas."<sup>2</sup> "Nothing," he further explained, "could be more consoling than the great piety of our poor people. All without exception approached the Holy Sacraments." "At my Mass on Sunday in the Cathedral," Moran emphasized in conclusion, "there were about 1000 men at Holy Communion." In the nearly thirty years that he faithfully served Rome in Ireland, Paul Cardinal Cullen not only reformed the Irish Church but, what was perhaps even more important, in the process of reforming that Church he spearheaded the consolidation of a devotional revolution. The great mass of the Irish people became practicing Catholics, which they have uniquely and essentially remained both at home and abroad down to the present day.

THE MEASURE OF CULLEN'S ACHIEVEMENT naturally depends on how much had been done to make practicing Catholics of the Irish people before

<sup>1</sup> J. P. Cooke to Kirby, Kirby Papers (hereafter K.), Archives of the Irish College, Rome.

<sup>2</sup> Dec. 19, 1877, K.

his arrival in Ireland in early 1850. What resources, in terms of plant and personnel, had been available to the Church for the encouragement and sustaining of devotional practices? And what was the character of as well as the example given by the Irish clergy to their flocks in promoting such practices? Corporately characterizing some 2,500 priests or even only some thirty bishops over a period of fifty years is obviously as hazardous as it is difficult.<sup>3</sup> And given the still raw state of the available evidence any systematic analysis of the resources of the Irish Church before the famine is as yet virtually impossible.<sup>4</sup> While the evidence is admittedly not in a condition, either quantitatively or qualitatively, to yield a consensus satisfactory to historians, it may be useful to attempt to structure a frame in which that developing body of evidence may be more intelligently researched and analyzed.

Since the quantitative problem of the number of clergy is relatively the easiest to come to terms with, perhaps it would be best to deal with it first. In 1800 there were about 1,850 priests, including some 26 bishops, in Ireland for a Catholic population estimated at 3,900,000, or roughly a ratio of one priest to 2,100 faithful. There were also in 1800 only 122 nuns in Ireland, which if reduced to a ratio divides out at the meaningless proportion of one nun to 32,000.<sup>5</sup> By 1850 the ratio between priests and people was still about one to 2,100, with over 2,500 priests available for something more than 5,000,000 Catholics. The nun population, however, had by 1850 increased thirteenfold, from 122 to over 1,500 in fifty years, and instead of one nun for 32,000 people, there was one nun for every 3,400 Catholics.<sup>6</sup> These figures, of course, in themselves are actually misleading because they mask the effects of the outstanding social tragedy in modern Irish history—the Great Famine. Between 1800 and 1840 the Catholic population had risen to 6,500,000, an increase of about 2,600,000, and probably increased another 250,000 by 1846.<sup>7</sup> By 1850, as a result of

<sup>3</sup> Emmet Larkin, "Church and State in Ireland in the Nineteenth Century," *Church History*, 31 (1962): 295–306.

<sup>4</sup> Emmet Larkin, "Economic Growth, Capital Investment, and the Roman Catholic Church in Nineteenth Century Ireland," *AHR*, 72 (1966–67): 852–84.

<sup>5</sup> Charles Vane, marquess of Londonderry, ed., *Memoirs and Correspondence of Viscount Castlereagh*, 4: 99, 172.

<sup>6</sup> *Census of Ireland, 1851* (Dublin, 1856).

<sup>7</sup> "First Report of the Commissioners of Public Instruction, Ireland," *Parliamentary Papers*, 1835, vol. 33, no. 45. The estimate of the Catholic population is based on the percentages of the various denominations given in this first religious census taken in Ireland in 1834: Population (total) 7,943,940; Catholic 6,427,712; Church of Ireland 852,064; Presbyterian 642,356; Other 21,808. According to these figures the Catholics made up nearly 81 per cent of the total population, while the combined Protestant total was about 19 per cent. If, therefore, the Catholic population in 1841 is estimated at 80 per cent of the census figure of 8,175,000 for the total population, the round number of Catholics is about 6,500,000. The Catholic populations of 3,900,000 in 1800 and 5,250,000 in 1850 are also based on taking 80 per cent of an estimated total in 1800 and a census total in 1851 respectively of 4,900,000 and 6,554,074, though both in 1800 and 1850 the percentage of Protestants to Catholics was more likely to have been somewhat higher than in 1834, the probable peak year of the Catholic population bulge in the first half of the nineteenth century. In 1861, for example, when for the first time the decennial census included figures for religious denominations, the Catholics accounted for some 78 per cent of the total population.

the famine and its aftermath, this population of nearly seven million in 1846 was reduced by some two million in four years.<sup>8</sup>

When it is realized that in 1840 there were only about 2,150 priests for a Catholic population of 6,500,000, or merely one priest for every 3,000 people, and that there were, furthermore, only about 1,000 nuns, or one for every 6,500 faithful, it becomes rather obvious that in the decade of the forties, and especially in the years before 1846, the Church in the face of incredibly adverse economic circumstances responded impressively, even if tardily, to the challenge of growing numbers by increasing the clerical population by some 400 priests and over 500 nuns—a twenty and fifty per cent increase respectively in ten years.<sup>9</sup> After 1840 and before the famine, therefore, the priests were gaining slowly and the nuns rapidly in relation to a population that was still increasing, though at a progressively declining rate. Patently, this relative increase in the clerical population meant little in practical terms before the famine, but when the population was suddenly reduced by nearly two million between 1846 and 1850 the whole clerical-lay numerical relationship was dramatically transformed, and what may have been only a short-term tendency rooted in a heroic institutional effort to increase the clerical population between 1840 and 1846, became in the next one hundred years a basic secular trend in Ireland. What emerges, then, even from this cursory analysis, is simply that before the famine any effective service on the part of the clergy was severely limited by the sheer weight of lay numbers, and that up to 1840, at least, the situation had been getting progressively worse.

But if the shortage of priests was so serious, perhaps the numerical deficiency was compensated for in some measure by the quality of their performance. Without more detailed biographical information about the nearly five thousand priests who served the Irish Church between 1800 and 1845, however, any estimate of their corporate character and conduct must remain largely impressionistic. There is, in fact, a strong bias in the available evidence in favor of extreme presentations. If, for example, one confined oneself only to reading the correspondence between Ireland and Rome in the archives of the Society for the Propagation of the Faith (*Propaganda Fide*), where nearly all the dirty Irish clerical linen was washed, the clergy might easily be characterized as drunken, disorderly, and immoral, or worse. While it is obviously impossible to present in any meaningful way more than fifty years of evidence from the *Propaganda* archives, perhaps a few examples will not only suffice to show what the nature of the evidence is, but also what the problems are in evaluating it. "I expect to leave this town tomorrow," Edward Dillon, the archbishop of Tuam, explained from Tuam in County Galway, on January 7, 1805, to John Collins, one of his priests, "and do not intend to return till Lent."

<sup>8</sup> *Census of Ireland, 1851.*

<sup>9</sup> *Census of Ireland, 1841* (Dublin, 1843).

"Previous to my departure," he warned Collins, "I cannot help reminding you of the advice I gave you when you were last at this house."

I am positively determined not to tolerate any whiskey drinking or other publick irregularities amongst my clergy. . . . Let me observe to you finally that if you wish to continue in Dunmore or to be employed in the ministry in this Diocese you must learn to sett a higher value on the sacred character with which you are invested than you have hitherto done. Particularly you must not be known to associate with such persons as Math<sup>w</sup>. Martin's or Martin's Strumpet's; much less should you church such persons. I often advised Frank Burke and his Co-ajutor he vainly thought that I would confine myself to unavailing advice, beware of meeting the fate of the former the later is tolerated merely thro necessity for a few months.<sup>10</sup>

"A Rev<sup>d</sup>. M<sup>r</sup>. Corbett, a priest of my Diocese," James O'Shaughnessy, bishop of Killaloe, reported to his agent in Rome from Ennis in County Clare, on November 27, 1815, "has been charged with adultery, and with having occasioned the separation of man and wife." "The case seemed so clear against him," O'Shaughnessy explained, "that he ought to have submitted and retired, but in place of doing so, he loudly called for a public trial."

During this trial more perjury and wickedness was practiced than in any Civil Court in the world. Some turbulent and disorderly priests made common cause with M<sup>r</sup>. Corbett, and I am informed they joined in a remonstrance to the Holy See, alledging that the sentence passed by the pious and learned D<sup>r</sup>. Wright was not founded in justice. My own humble opinion is that there was already too much of this shocking business, and that our Holy Religion would be less-wounded, and less scandal given, by not stiring the embers further.

The unfortunate woman seems within one month of her accouchment tho her husband left the country 15 months ago. If with your usual attention you would make known the circumstances to the Sacred Congregation, and put a stop to any further proceedings, it would be rendering an essential service to the Catholic Religion of this poor persecuted country.

"When anything final," O'Shaughnessy suggested, "comes to your knowledge I request to hear from you." "I am sure," he concluded encouragingly, "you will give it every necessary attention, of which I will be *mindful*."<sup>11</sup>

The third and final example of the nature of the evidence in the Propaganda archives concerning the conduct of the clergy is a long letter from James Murphy, bishop of Clogher, to Lorenzo Litta, cardinal prefect of Propaganda, dated Monaghan, April 2, 1818, thanking His Eminence for the news of the appointment of a coadjutor with the right to succeed him in Clogher, but also taking exception to the cardinal's suggestion that he should be less severe regarding several of his priests with whom he was in serious litigation at Rome. "And now permit, My Lord,"

<sup>10</sup> *Scritture riferite nei congressi, Irlanda*, 18, fol. 316.

<sup>11</sup> *Ibid.*, 20, fol. 69.



Murphy began his peroration politely, “with the most profound deference, however, for your Eminence and the Sacred Congregation, to say a few words on the treatment I conceive myself to have received from the Sacred Congregation,

Your Eminence may recollect, that in the year 1814, when heavy charges were preferred against me to His Holiness, and two appeals lodged against me, one of them by Priest Maginn, since deceased, on account of having suspended him for *Turpia in Tribunali*; the other by Priest Goodwin having published to the world, and preferred and inscribed to me a *Libellum Accusatorium*, in which he charged a highly respectable and zealous priest with having revealed the confession of his penitent, and in which he also charged two other pastors, the most respectable in the Diocese, with having cooperated in said wicked act. At that time your Eminence sent two commissions to my then metropolitan, the most Rev<sup>d</sup> Richard O’Reilly, the one to examine narrowly into the said charges and report the result to his Holiness; the other to try the appeals and pass a definitive sentence on them—Both these commissions my metropolitan executed, and after a strict scrutiny into the charges against me, he told me, he reported them unfounded and calumnious—relative to the two appeals, he pronounced definitively, as empowered, that my suspensions were just and necessary in both cases—Now what I feel for and consider *severe*, is, that I, or, indeed, any other bishop should be exposed, dragged publickly and shamefully from tribunal to tribunal and tried a second time on matters that were already definitively disposed of: for though my metropolitan erred in not depriving Priest Maginn of his parish, yet, his sentence, which bound that unfortunate man on oath never to hear the confession of a female, not only justified but even proved the necessity of my suspension—to these matters I beg leave to add, that your Eminence sent a commission to my metropolitan in the year 1816, in consequence of an appeal lodged by a Priest Duffy, a curate, and, of course, without any ordinary jurisdiction, against me, for my having interdicted him from exercising certain pastoral functions in despite of his parish priest, and for having suspended him afterwards for his contumacy, in not attending citations I sent him to appear before me, and account for his exercising all and every pastoral function in defiance of my interdict, and for his, besides, raising the people in open rebellion against their lawful pastor—Your Eminence, I say, sent such commission to my metropolitan with orders to cite the parties, and after hearing us, to report the result, together with his own opinion, to your Eminence—I, of course, obeyed the citation though labouring under infirmities and having upwards of sixty Italian miles to make, and having besides to bring necessary witnesses with me at heavy expenses, some thirty, some eighty and some an hundred miles. My metropolitan, after examining me and my witnesses in the presence of the appellant, called on him to rebut what I had proved, when he was answered by the appellant in a highly disrespectful and taunting tone, that he would not, nor would he, he said, answer a single question that he put him—my metropolitan made, of course, his report on this obvious and self evident case, and the Sacred Congregation, notwithstanding, gave it in charge a second time to the Visitor Apostolic—This I confess, I consider also *severe*.<sup>12</sup>

“It has exposed me,” Murphy maintained, “to additional heavy expenses, for the appellant, Priest Duffy, nor indeed any one of the other appellants,

<sup>12</sup> *Ibid.*, 21, fols. 158–59.

though they brought on the suits never paid a single sou of the expenses attendant on the different commissions." "It has, besides," he declared further, "by putting off and prolonging the decision, given them the assurance to expose and villify me and my administration frequently, in one of the most publickly circulating journals in this kingdom, and of threatening me openly and repeatedly with civil suits; so that I may justly say with the Apostle, *Spectaculum Facti Sumus* &c—" "All this publick abuse," Murphy concluded dryly, "I bore without an answer in the hope that God will give me an account for it [in] a better world."

If, on the other hand, one turns from the Propaganda archives to a perusal of the various pious lives of the Irish clergy for the same period, the result is simply a hagiographical headache, or worse.<sup>13</sup> The truth, alas, is not even found by invoking that favorite and prudent device in such circumstances—the *via media*. What happened between 1800 and 1845 is that the character and conduct of the clergy, which certainly left a great deal to be desired at the beginning of the period, was gradually and uniformly improved. By 1830 the worst was over, since the Irish bishops with the help of Rome finally secured the upper hand over their priests.<sup>14</sup> From 1830 the improvement, though still uneven, depending as it did on the character and strength of will of each individual bishop, was at all events steady. The improvement, however, does not appear to have been simply a function of the bishops' authority in time; it seems to have been a reflection of geographical circumstances as well. The improvement was most rapid and sustained in the ecclesiastical province of Dublin, while the deportment of the clergy in Cashel, Armagh, and Tuam was less and less satisfactory from one to the next.

On the occasion of the funeral of the archbishop of Dublin, Daniel Murray, at the end of February 1852, William Meagher delivered an oration in which he reflected on the practical improvement of the Catholic population of Dublin. He graphically described the conditions prevailing in Dublin some forty-two years earlier when Murray had been raised to the episcopal dignity.

<sup>13</sup> A critical bibliography of the numerous pious lives of Irish bishops, priests, monks, and nuns is also beyond the scope of this article. An honorable exception to this general charge of hagiography, however, must be noted in W. J. Fitz-Patrick's very fine Victorian, two-volume biography, *The Life, Times and Correspondence of the Right Rev. Dr. Doyle, Bishop of Kildare and Leighlin* (Dublin, 1880). In setting a lower limit in this biographical spectrum, the best example is perhaps Peadar MacSuibhne's more recent three-volume effort, *Paul Cullen and His Contemporaries* (Naas, 1961–65). For a partial list, at least of those biographies that range between the upper and lower limits set above, see the bibliography in T. J. Walsh, *Nano Nagle and the Presentation Sisters* (Dublin, 1959).

<sup>14</sup> The milestone in effecting the better conduct of the clergy by increasing the authority of the bishops was the simultaneous holding of diocesan synods in the four dioceses that made up the province of Dublin in the third week of July 1831. For an account of the background to the meetings, see William Meagher, *Notices of the Life and Character of His Grace, Most Rev. Daniel Murray, Late Archbishop of Dublin* (Dublin, 1853), 128–31; for the legislation of synods, see R. T. McGhee, *Diocesan Statutes of the Roman Catholic Bishops of the Province of Leinster* (London, 1837); for an excellent account of a reforming bishop, James Doyle (1819–34), see Fitz-Patrick, *Life, Times*, 1: 101–32.

The morals of the people of Dublin, Catholics among the rest, were hideously corrupted. The riches daily scattered through her streets in handfuls, to purchase the luxuries of an opulent, and profuse, and dissolute aristocracy; the easy and plentiful earnings of flourishing manufacture, and of extensive and successful commerce, were seized every hour, through a series of years, for indulgence of vilest libertinism, and wildest extravagance. Vices, too gross to be more than alluded to, stalked through the streets shamelessly—the drunkard raved without obstruction, and the blasphemer shouted his impiety, and the gambler squandered in nights of dissipation what his days of toil had accumulated. And, strange to say, and suggestive of many a sad and solemn reflection, there was in our city as large an amount of physical wretchedness, particularly among the lower ranks, then as now—as much squalid poverty—as much shivering nakedness—as much famine-stricken emaciation—as many ruined families—as many houseless orphans! Vice did more to fill the town with the agonies of human suffering than famine, and plague, and abject poverty have wrought in these latter days of woe. Flatter not yourselves, My Brethren, that these excesses and their direful effects were confined to sectarians; they were as rife, if not more so, amongst ourselves. Nor, unless by some standing social and religious miracle, could it be otherwise. Amid opportunities so numerous—examples so seductive—temptations so violent—with but a handful of clergy and a dozen small, mean, and incommodious chapels to second the proverbial faith and innate pious tendencies of the people, what wonder that the multitude was hurried away in this torrent of iniquity? And the mortifying truth is, that in Dublin, at the period alluded to, amid many Catholics there were but few practical Christians; very few whose lives supplied that substantial and only unerring proof of profitable attachment to the faith—the constant and regular frequentation of the holy sacraments. As the climax of her griefs religion had to weep for the first time, perhaps, in this land, over the faltering fidelity and submission of many a son, led astray by the phrenzy of recent revolution, and the false liberality of the day, and the desolating philosophism of France.<sup>15</sup>

There appears to be, moreover, a correlation between the conduct of the clergy in these ecclesiastical provinces of Dublin, Cashel, Armagh, and Tuam and their relative wealth as well as the extent to which each was urban and rural in terms of Catholic population. Furthermore, though the evidence is still very sketchy, the clergy appointed to the town parishes appear to have been better educated than those assigned to the country parishes.<sup>16</sup>

<sup>15</sup> Meagher, *Notices*, 11–12.

<sup>16</sup> "Report from the Lords Committees appointed a secret committee to enquire into the State of Ireland," 1825, *PP*, vol. 7, no. 521, pt. 2, Minutes of Evidence, p. 569. Mortimer O'Sullivan, a Church of Ireland clergyman, was asked, "Are you acquainted with the early habits of life, of persons who afterwards become Roman Catholic priests?—Yes. / From what class of life are they generally taken?—I think generally speaking from the lower orders. My connection with an endowed school, gave me an opportunity of knowing more particularly. / Of course you have been acquainted with some who have afterwards gone to Maynooth?—Yes. / Have you had any occasion to observe what have been the effects of a collegiate education upon those persons?—It appeared to me to leave them, with respect to their moral qualities and their political prejudices, just what they were before they had gone there. When I speak of the lower orders, I mean the poorer classes; there are some of a higher order; and that church shows great judgement in disposing of her clergy; those of better manners and better information are generally placed in the towns: and those persons who are from their habits and from their education less fitted to appear in public, are left in the country parts."

But the application of episcopal authority, the relative wealth of the Catholic communities, the extent of urbanization, and the educational level of the clergy were not the only determinants of social behavior. The moral and social values of the community and the pressure the community applied in terms of what it considered to be right or wrong also affected clerical conduct. The principal vices among the clergy were drunkenness, women, and avarice. Interestingly enough, while this seems to be the order of their importance among the bishops in their efforts to impose discipline, it does not appear to be the order of their importance either before or after the famine as far as the laity who cared were concerned. Among a land-hungry and poverty-stricken peasantry avarice was the deadliest of the deadly sins, while lust and drunkenness were viewed with a more understanding, even if disapproving, eye.<sup>17</sup> The seriousness of the problem of clerical avarice vis-à-vis the faithful, for example, was certainly reflected in early nineteenth-century Ireland in the need of the bishops of the province of Dublin to set up by statute a uniform tariff for clerical dues at their diocesan synods in the summer of 1831. The tariff, however, not only gives a comprehensive glimpse of what was thought to be a fair and proper remuneration for the various services rendered by the clergy but also details an interesting summary of the clergy's sources of income.

1. Baptism—shopkeepers and farmers .....	0	5	0
Do. poor labourers .....	0	2	6
2. Marriages—shopkeepers and farmers .....	2	0	0
3. Licenses for do .....	0	10	0
Marriages for poor labourers .....	1	0	0
Licenses for do .....	0	5	0
4. Masses for dead sung, to shopkeepers and farmers—parish priest .....	0	15	0
Every other priest .....	0	10	0
Any other priest who does not officiate .....	0	5	0
5. Masses, not sung .....	0	10	0
Masses for dead, to poor labourers .....	0	5	0
6. Private masses .....	0	2	0
7. Collections after marriages—these must be voluntary according to the stations of the parties, they may vary from £1 to £100, or more.			
8. Dues at stations—these Dr. Murray informs us are the chief support of the priests; they cannot be called voluntary, for custom makes them compulsory; they are contributed by every person who can give anything, and vary from one shilling to five, as the Editor is informed, say the lowest average from the population of Ireland who attend stations and confessions .....	0	1	6

<sup>17</sup> The problem of exorbitant clerical dues and the resistance of the laity to them had been an issue in Ireland from at least the latter part of the eighteenth century. See Robert E. Burns, "Parson, Priests and the People: The Rise of Irish Anti-Clericalism, 1785-89," *Church History*, 31 (1962): 151-63.



9. Fortuitous emoluments—of these, at least one great source of revenue, is masses for delivering the souls from purgatory of those who are dead and buried, left either by will or given by relations of deceased, or, what is very common, given by the poor creatures themselves, for masses before they die, and to benefit their souls when they are gone,—of these it is impossible to calculate the amount.
10. The collecting of corn from the people. This is sometimes commuted for money, and is valued at 1s. 6d. per house, or more.<sup>18</sup>

More particularly, the parishioners from Kilcommen Erris, near Belmullet in County Mayo, in the diocese of Kilalla, petitioned the pope in early 1840 about the abuses to which they were subject from the local clergy. Their petition comprised some fifteen heads of complaint, and like so many other documents in the Propaganda archives it was the product of a fierce local struggle for power, with the contending parties prepared to say and to write the worst about their opponents. The crucial aspect of this document, therefore, is not indeed whether the charges made in it were necessarily true, but rather that the charges made in it are a serious comment on what the mores and values of the person or persons who wrote it actually were.

To Our Most Holy Father in God Pope Gregory the XVI Successor of Saint Peter and Vicar of Jesus Christ upon Earth. . . .

6thly that the poor uneducated peasantry of the parish generally feel not only scandalised but actually horrified at the not merely tyrannical, but unchristian like conduct of the Rev<sup>d</sup> Mr. Conway towards them during the Confessions. When a poor, but pious, humble, contrite penitant presents himself before the altar of reconciliation bewailing his offences, and with devout compunction soliciting to be admitted to the Paschal or Christmas distribution of the Bread of Life—if he has not money to propitiate the avaricious ire of the Rev<sup>d</sup> Mr Conway, he is not quietly dismissed as being too poor and contemptible for enjoying the Celestial benefits; but he is scolded, villified and threatened “D’air Cunnial De Mur Sagart”. “By the obligation of God as a Priest,” he will have revenge, if it were to run for seven, ten, fifteen, or 21 years. . . .

10thly that old Anthony Burke who lives with his daughter and son in law in Muinnaba, and whose aged wife lived with another daughter in Claggeh—did upon the death of his wife offer 2<sup>s</sup>/6<sup>d</sup> to the Rev<sup>d</sup> Mr. Conway to have mass said for the soul of his departed wife;—but that Mr. Conway not only refused the money but in a paroxysm of violence proclaimed Burke from the Altar, did ring the bell with rage—and invoke a horrible imprecation upon him and his worldly substance, for offering him 2<sup>s</sup>/6<sup>d</sup> to say Mass for the poor woman though no priest before Mr. Conway charged more than one shilling. The result was, that the Congregation would have slaughtered him [“him” crossed out] each other were it not for some peaceable characters who mollified the rage of the exasperated people. . . .

<sup>18</sup> McGhee, *Diocesan Statutes*, xli–xlii.

12thly we have had, through the pious zeal of Rev<sup>d</sup> Neal McNulty, the walls of a good chapel 80 feet in length and 30 in breadth built for the last sixteen years, and not withstanding the number of clergymen that passed through this parish during that period, and collected great sums of money from us, for the ostensible purpose of roofing the Chapel, yet they have taken away our money and left us these sixteen years without a temple of worship to put our heads into or to screen us from the inclemency of the weather and although Dr Feeny has been appealed to against these clerical plunders, he has not ordered the money to be refunded to James O'Donel Esq<sup>r</sup> the Treasurer of our Chapel Committee.<sup>19</sup>

In order to sum up here, however, on the subject of clerical avarice, which is yet another subject, a long account of the situation in Ireland by T. Chisholme Anstey, an English Catholic, apparently to the secretary of Propaganda, Giovanni Brunelli, from London on November 17, 1843, is certainly worth our attention. In his account Anstey, who appears to have been well acquainted with the clergy and conditions in the province of Connaught, maintained,

it is well known in every part of Ireland with which he is acquainted, and to the best of his belief also in other parts thereof, that however well disposed a parish priest or curate may be to relieve his parishioners or some of them from grievous and oppressive payments of the kinds specified ["Tithes, church dues, oblates, stock fees, money for repairs of fabric"], the relatives, (often very numerous) of such ecclesiastics are certain to obstruct the concession by clamorous complaints and remonstrances against his unkindness to his own flesh and blood, who by his ill timed liberality he is defrauding of their hopes of succession to his property after his death and of occasional contributions out of the same during his lifetime, and to which succession and contributions they in the popular opinion as well as in their own have a kind of equitable claim, founded upon the consent, which his family is supposed to have given in the first instance to his being withdrawn from field labor and domestic service in order to go to the seminary; and, that the fear of such complaints, remonstrances and appeals to popular opinion hath the effect of making the priests to be watchful and austere in the exact and undeviating levy of their aforesaid dues, is apparent from the greatness of their incomes; that is to say in Connaught, which is the cheapest part of Ireland, and where money is twice as valuable as it is in London, there are very few parish priests, if any, whose incomes are less than 200 sterling per annum although not one farthing of such incomes is appropriated by either priest or proper to any other purpose than the mere support of the priest. But in most parishes the income is very much higher and ranging to £500 and upwards per annum; insomuch that it is a vulgar and proverbial saying throughout Ireland that the best or richest matches are to be had with the kindred of priests and that their farms are certain to be well stocked and furnished.<sup>20</sup>

<sup>19</sup> *Scritture riferite nei congressi, Irlanda*, 28, fols. 634-35. The petition is signed by James O'Donel as "Chairman of the Parochial Committee" and Hugh Joseph O'Donel as "Secretary." Three other petitioners signed their names, but nineteen others made their "X" mark and were signed for.

<sup>20</sup> *Ibid.*, fols. 121-45.

Anstey then went on to explain that there was a fixed tariff on burials, masses for the dead, churching women, etc., below which an offering might not fall, but might exceed it. Baptisms, he reported, were 2s. 6d., while marriages were a pound or a guinea, in addition to the money collected for the priest at the marriage feast. One such collection, Anstey noted, was £25, though no one there was above the rank of farmer. If the people involved were poor, Anstey admitted, the clergy would perform the required service gratis, but in order to test the truth of their allegations of poverty, the priests were in the habit of announcing in the chapel, church, or meeting house the names of those who had gratuitously received their services. "And, however poor the Irish peasant may be," he added, "he is rarely disposed to accept the exemption upon such humiliating conditions." The result of all of this had been, Anstey continued, that secret societies had been formed, and "by means of such associations they have from time to time endeavoured, and still are endeavouring, to compel the priests to agree to a more moderate tariff of dues and to compel the people to abstain from paying them any dues except according to the tariff appointed or proposed." Anstey then turned to the bad Irish habit of "stations," where the priest would designate the houses of various parishioners who were relatively well off as the place where he would hear confessions and say mass that week for all those in the immediate area. He complained that mass was being offered in "cabins" rather than in "chapels" because the fees were greater. The "station," moreover, he pointed out, was obliged to offer hospitality and had to cater to the priest's choice of tradesmen and victuallers. The Irish priests, Anstey further complained, not only did not keep to the rubric and practice of Rome in the Mass, but their sermons were of poor quality, and "the ignorance of the people in matters of Religion is frightful, and, in particular, that the doctrine of the Trinity is rarely known or ever heard of among them, much less the doctrine of the Real Presence and other articles of Faith." After all this and a good deal more Anstey finally concluded by advising the Propaganda that the only hope for religion in Ireland was for the pope to send a legate with power to correct the many abuses. Clerical avarice, however, legate or no, whether in terms of the clergy farming or grazing large tracts of land in their brothers's or nephews's names or in squeezing the people for alms and dues was, after drunkenness, the most difficult of the deadly sins for either the bishops or the laity to check.

BUT HOW MANY of the laity really cared? The best way to begin to answer that question is to determine how many people actually attended church. While it is certainly true that all those who attended mass religiously were not necessarily active in the concerns of their church, knowledge of

the numbers who attended is at least helpful for setting an upper limit on those who were concerned. The figures on church attendance in pre-famine Ireland indicate that only thirty-three per cent of the Catholic population went to mass.<sup>21</sup> This is all the more remarkable in that in something less than fifty years church attendance would increase to over ninety per cent, and so it has continued down to the present day.<sup>22</sup> Why attendance was relatively so low in pre-famine Ireland is obvious. There were not enough priests and there were not enough churches, or, more particularly, not enough seating space in the existing churches to accommodate those who might be inclined to attend to their religious duties. If, for example, all the priests in Ireland celebrated the two masses they were allowed on a given Sunday in 1840 there would have been 4,300 masses for 6,500,000 people, or one mass for every 1,500 people in attendance, and there were no chapels and very few churches in pre-famine Ireland that would accommodate a thousand worshippers.

This deficiency was offset to some degree before the famine by the widespread practice of "stations." Baptism and marriage were also frequently celebrated in private houses rather than in churches. These practices were generally frowned on by those who were attempting to reform both clergy and laity and increase devotional zeal. The complaints of the reformers, who were concerned about the abuses attendant on the system, had mainly to do with the exorbitant "offerings" extracted by the clergy for the administration of the sacraments and the undignified if not unholy celebration of sacred rites in profane places. James Maher, writing from Carlow to his nephew Paul Cullen in Rome in early January 1842, asked "Could not Rome do something to stimulate the zeal and watchfulness of the Bishops: the holding of Stations for Mass and Confession at private houses is the very worst system. Wretched filthy cabins have been lately honored with stations." "The people," he explained,

cannot be instructed. The Priest no matter how zealous cannot do his duty. The young clergyman is brought into contact with his female penitents. The result is confessions are often invalid or sacrilegious. It is almost impossible that the poor country people in the circumstances could disclose their sins. Struggling with their natural reluctance to avow their guilt, and fearing at the same time to be overheard by those who are pressing around the Priest, who cannot utter a word of encouragement to the sinner, except in the lowest and therefore intelligible [*sic*] whisper that can be expected.

"Could not Rome," Maher suggested again, "induce the Bishops to change the system? Stations in the chapels have been recommended in the Statutes

<sup>21</sup> David W. Miller, "Religion and Social Change in Pre-Famine Ireland," unpublished paper, p. 3. Since the writing of his paper Professor Miller has revised his estimate of church attendance by Roman Catholics upward to about 40 per cent, but he is still "prepared to state flatly that the prevalence of extraordinary religious devotion evidenced by extremely regular church attendance is a post-Famine phenomenon."

<sup>22</sup> Jean Blanchard, *The Church in Contemporary Ireland* (Dublin, 1963), 29-31.

for this province. But the recommendation has proved a dead letter." "We owe much to Rome," he assured Cullen in conclusion, "and if she would help us to this reform, we would be more deeply her debtor."<sup>23</sup> Nearly all the synods, provincial and national, between 1830 and 1875 had statutes disapproving of "stations," and even though Rome eventually added her proscriptio as requested by Father Maher, the practice died very hard, especially in the south and west where it still survives in some places.<sup>24</sup>

Before the famine, then, despite severe limitations in plant and personnel, there was a small but perceptible change and increase in devotional practices in Ireland. Why this was so had a great deal to do with the enthusiasm and hope generated by the moral and political reform movements of Father Mathew and Daniel O'Connell. Both the Total Abstinence Society and the Repeal Association grew up in a period heavy with gloomy forebodings of impending disaster as bad harvest succeeded bad harvest, prices for foodstuffs continued to fall rapidly, and emigration mounted. Literally millions took Father Mathew's temperance pledge as the production of Irish whisky fell from 12,296,000 gallons in 1839 to 5,546,283 gallons in 1844. As Daniel O'Connell enrolled the "nation" in his Repeal Association his "monster meetings" numbered in the hundreds of thousands of people.<sup>25</sup> Essentially these were both revival movements, which created not only an enormous enthusiasm but, because of the underlying anxieties created by population pressure and land hunger, also contributed greatly to an already heavily charged emotional atmosphere. In the early 1840s, therefore, there already were manifest signs of that devotional revolution, which Paul Cullen would proceed to help make and consolidate some ten years later when he would arrive in Ireland as archbishop of Armagh and apostolic delegate.

On the occasion of a papal jubilee in 1842, for example, when the penitents were offered special indulgences if they would but confess their sins and come to Christ, the bishop of Cork wrote to Cullen in Rome asking the rector of the Irish College to secure for him additional faculties to dispense in terms of sins especially reserved to the pope. "Sinners," John Murphy explained to Cullen in May 1842, "who have for years lived in fornication, adultery, incest & have recourse to the tribunal of penance" are coming in in droves. "I have a melancholy list of 64 couples," he added sadly,

who in obedience to my commands have separated *a thora*; for where there is abject poverty, with a numerous brood we cannot insist on separation *a mensa*. The Jubilee is open in only one Parish, how numerous will be the blacklist before the conclusion of it in the whole diocese—it would be endless to enter into the minute details of every case.<sup>26</sup>

<sup>23</sup> Jan. 2, 1842, Cullen Papers (hereafter C.), Archives of the Irish College, Rome.

<sup>24</sup> Blanchard, *Church in Contemporary Ireland*, 32.

<sup>25</sup> Sir James O'Connor, *History of Ireland, 1798-1924* (London, 1925), 1: 301; L. J. McCaffrey, *Daniel O'Connell and the Repeal Year* (Lexington, 1966).

<sup>26</sup> May 29, 1842, C.

Some nine months later James Maher again reported to his nephew in Rome from Carlow. "I forgot, tho I intended it to tell you of the wonderful success of the Missionaries in Athy." "A visit from Father Mathew," he explained, "would not have put a greater number in motion. Hundreds remained all night in the Chapel, and many remained in town away from their homes from 5 and six days waiting an opportunity of confessing." "This extraordinary movement," Maher further noted, "has confirmed an old opinion of mine that we do not always afford the people an opportunity of general confession when required. In fact we have not half Priests for the wants of the Mission, and a very considerable proportion of the Parrochi leave the confessional almost entirely to the curates." The following August Cullen's sister Margaret informed him that they had had the "Missioners" in Carlow town for the last five weeks, and that it "would be impossible for me to describe the enthusiasm of the people." If the missioners were angels from heaven, she added, they could not be more venerated. Work was at a standstill, while people followed them around all day and crowded "in *hundreds* to the Confessionals, many very many who had never before been there." The missioners preached three times a day in the chapel, which was "crowded to suffocation." "What a pity," she finally concluded, "we have not more Priests in the Parish. I fear a great deal of their labours will go for nothing. Where is the opportunity for the *bulk* of the parish to approach the Holy sacraments."<sup>27</sup>

Indeed, the "bulk" of the Irish people in the 1840s never did have the opportunity to approach the sacraments. In writing his customary annual letter in October 1851 to the cardinal prefect of Propaganda, for example, Michael Jones, a former student of the Propaganda's Urban College in Rome, complained about "an almost general neglect in giving the People the necessary knowledge of the Faith, the Commandments and the Sacraments." "The Irish People," Jones explained to Cardinal Frasoni, "are very good, but much neglected in every way by both the Civil and Ecclesiastical Government, more by the latter than the former." It was time "for the Ecclesiastical authority to put an end to the present state of things." The priests who received training at the National Seminary at Maynooth, did "not receive in the College any notion how things should be. The old system of the days of Persecution, the Catacombs, and the Caves is all that they know."<sup>28</sup> That the Irish people were receptive and might

<sup>27</sup> Feb. 21, Aug. 30, 1843, C.

<sup>28</sup> Michele Jones to Giacomo Cardinale Frasoni, Oct. 9, 1851, *Scritture riferite nei congressi, Irlanda*, 30, fols. 720-21, Archives of the Society for the Propagation of the Faith, Rome. The original letter is in Italian. See also K., Feb. 28, 1858, for a letter from John Kyne, chaplain at Alum Bagh, Lucknow, Oude, East India, to Thomas Grant, bishop of Southwark (London) and Catholic chaplain-general, regarding Kyne's work among the British troops, who were mainly Irish and presumably Catholic. "I am anxious first of all to communicate to your Lordship the pleasing fact that even here in India I can bear testimony from personal experience, to the good effect produced by the Mission given last year to the soldiers at Chatham. In fact the recruits, who arrived here last month from Chatham are the *only* persons of whom it can be



have made excellent evangelical material to work with is certainly given credence by the unusual success of the sporadic attempts made, but the resources available for a religious and moral revival on a national scale were too slender in the face of the number of people.

What achievement there was before the famine, then, was largely confined to that "respectable" class of Catholics, typified by the Cullens and the Mahers in Carlow, who were economically better off. Since this class generally survived the famine intact, while the "bulk" of the cottiers, laborers, and paupers were swept away by starvation, disease, and emigration, the Church actually had a stronger devotional nucleus relative to absolute numbers in 1850 than in 1840.<sup>29</sup> This nucleus, furthermore, would come to count for more with every succeeding year because the remaining subsistence population was gradually liquidated by a continuing emigration sparked periodically by the fear of starvation. When Paul Cullen arrived in Ireland he therefore had a potentially more favorable situation than has been generally supposed. He also patently derived very great advantage from the psychological impact the famine had on those who remained in Ireland. The growing awareness of a sense of sin already apparent in the 1840s was certainly deepened as God's wrath was made manifest in a great natural disaster that destroyed and scattered his people. Psychologically and socially, therefore, the Irish people were ready for a great evangelical revival, while economically and organizationally the Church was now correspondingly ready after the famine to meet their religious and emotional needs.

THE PROBLEM OF CHARACTERIZING the making and consolidating of this devotional revolution is somewhat simplified by the fact that the period begins with the first National Synod of Thurles in 1850 and ends with the second National Synod of Maynooth in 1875. The first Synod of Thurles was primarily concerned with the proper administration of the sacraments

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said, that they complied with the Easter obligation. With this exception, the rest of the poor soldiers were in a most pitiable state. The vast majority had never in their lives received the Holy Sacraments. And their ignorance of even the first principles of religion was truly astonishing with all my experience I was never in my life so taken by surprise. If I had heard it from others, I could not have believed it possible. Yet with all that they are not bad materials to work upon—I believe, notwithstanding the general impression to the contrary, that on the whole as much may be made of them as of any other class of Christians." By way of proof Kyne explained that in three months he had brought upward of a thousand to the sacraments and that every morning he had between thirty to fifty communicants, while every evening he recited the rosary and gave religious instruction.

<sup>29</sup> John Kepple to Kirby, Sept. 2, 1852, K. Writing from Ballyhea, Charleville, where he was parish priest. Kepple noted, "There is not in the County of Cork a finer country than this. The population of my parish is not very large, the poorer portion of it (as everywhere else through out Ireland) has been swept away by the Famine fever emigration &c &c. The farmers tho' not numerous are very respectable, and comfortable but in consequence of the failure of crops, and the thinness of the people our emoluments here are inconsiderable, however I don't complain."

and regulating more closely the lives of the parish clergy.<sup>30</sup> In the statutes the clergy were exhorted to administer the sacraments more often and only in church, except where it was impossible, and to encourage the laity to better lives by the clergy's own good example. The bishops were assigned the responsibility by the synod of seeing that these reforms were carried out in their respective dioceses. Twenty-five years later the Synod of Maynooth reiterated mainly what had already been decreed in 1850 and in enlarging upon the statutes further increased episcopal control and authority.<sup>31</sup> The making of the law, however, proved to be one thing, and the enforcing of it quite another. The first Synod of Thurles had made it quite clear that the Church was to be reformed from the top down and that the responsibility for enforcing that reform should fall to the bishops. As apostolic delegate, however, Cullen had very real difficulties in keeping the Irish bishops up to the mark. The problem was not only that a large number of bishops were set in their ways and naturally averse to reform, but that the bishops also had serious differences with regard to educational and political matters, and their quarrels in these areas seriously inhibited their efforts at pastoral reform.

Cullen, however, was not only a reformer but a very effective ecclesiastical politician, and with the support of Rome, especially in episcopal appointments, the Irish Church was reformed by him in his generation. His method was to deal with one principal issue at a time, while trying to contain the worst effects of the other issues. His fellow archbishops of Dublin, Tuam, and Cashel, for example, each respectively represented educational, political, and pastoral problems for Cullen. He mobilized Tuam and Cashel against the educational policies concerning the Queen's Colleges, advocated by the venerable archbishop of Dublin, Daniel Murray, who was strongly supported by a minority of the bishops. When Murray died in the winter of 1852 Cullen was translated by Rome from Armagh to Dublin and his nominee appointed to replace him in Armagh. The opposition among the bishops, without the effective leadership of Murray, was quickly isolated and eventually crushed. Cullen then turned to the problem posed by the archbishop of Tuam, John MacHale, and the involvement of the clergy in secular politics, particularly the Tenant League. Supported by Armagh and Cashel, and with Rome's aid, MacHale was also eventually isolated, and if not crushed, he was at least effectively contained. Finally Cullen tackled the problem of pastoral reform in the province of Cashel, which was most difficult because most of the bishops of that province strongly supported the custom of "stations." Cullen once again undermined the opposition to him by having only those who agreed

<sup>30</sup> *Decreta, Synodi Nationalis Totius Hiberniae Thurlesiae Habitae Anno MDCCCL* (Dublin, 1851).

<sup>31</sup> *Decreta, Synodi Plenariae Episcoporum Hiberniae, Habitae Apud Maynutium, An. 1875* (Dublin, 1877).


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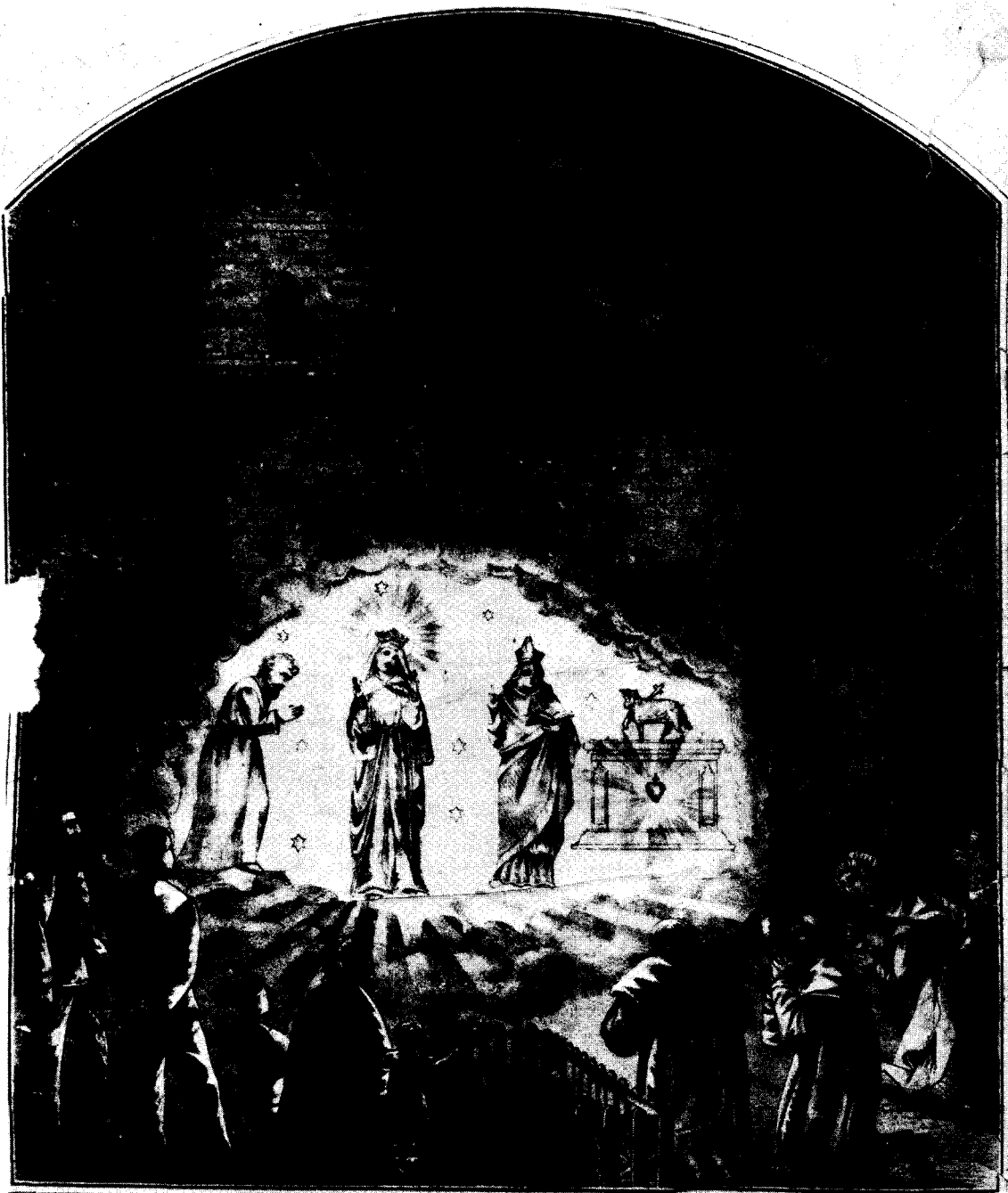
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## THE APPARITION AT KNOCK CO MAYO.

AS SEEN ON AUGT 21ST 1879. (EVE OF THE OCTAVE OF THE ASSUMPTION)

MANY MIRACULOUS CURES HAVE BEEN EFFECTED THERE SINCE THE ABOVE OCCURRENCE.

THIS VIEW WAS TAKEN ON THE SPOT BY W. COLLINS.

AND SUBMITTED TO, AND APPROVED OF BY THE SEVERAL PERSONS WHO SAW THE ABOVE

About 7:30 on the evening of the 21st of August, 1879, the vigil of the octave day of the Feast of the Assumption at the parish church of Knock, Co. Mayo, "an apparition of Our Blessed Lady, wearing a large brilliant crown and clothes in white garments was distinctly seen by some fifteen persons at the south gable wall of the church. Our Lady is described as having her hands raised as if in prayer and her eyes turned towards heaven. On her right hand was St. Joseph, his head inclined towards her, and on her left was St. John the Evangelist, attired as a bishop, his left hand holding a book and his right hand raised as if in preaching. To the left of St. John was an altar on which stood a cross and a Lamb, about 8 weeks old. . . . The gable wall where this manifestation was seen was covered with a cloud of light and the vision lasted for fully two hours." Liam Ua Cadain, *Venerable Arch-Deacon Cavanagh, Pastor of Knock, 1867-1897* (Dublin, 1955), 69. Illustration courtesy of National Library of Ireland.

with his reform principles succeed to bishoprics in that province, and gradually but relentlessly those bishops who were reluctant to change their ways in the rest of Ireland were replaced by Cullen's more energetic and aggressive nominees. By 1875, therefore, there was hardly a bishop in Ireland, except MacHale, who did not zealously promote pastoral reform in his diocese, whatever his educational and political views were. Actually this resolution of the distribution of power in the Irish Church in favor of Cullen was not nearly as smooth or inevitable as it may appear from this oversimplified account, but what is most important to understand is that this resolution of power was absolutely necessary to the making and consolidation of the devotional revolution that took place.

In the twenty years following Cullen's arrival in Ireland the number of priests was increased by some seven hundred, or nearly twenty-five per cent, to a total of about 3,200, while the Catholic population declined from five to four million, or a ratio increase of one priest to 2,000 people to one priest to 1,250 people in 1870. The nun population increased even more rapidly over the same period. In 1850, for example, there were only some 1,500 nuns in Ireland, while in 1870 there were more than 3,700, or an absolute increase of 2,200, and a ratio increase of 1:3,300 in 1850 to 1:1,100 in 1870.<sup>32</sup> Not only were the numbers of clergy relative to the population rapidly appreciating, but it also appears that their quality was improving over the same period. The amount of dirty clerical linen washed in Rome appears to have decreased, as did the volume of litigation between the bishops and their priests. The improving quality of the clergy, moreover, is not only testified to by their really prodigious energy in building churches, schools, seminaries, convents, and parochial houses, but their conduct and learning was certainly improved by conferences, retreats, synods, and the erection of cathedral chapters, as well as by the annual or triennial visitations by their bishops depending on the size of their dioceses.

In extending their increasing zeal and piety to the laity the clergy centered their attention on the sacraments, and especially on the sacraments of penance and Holy Eucharist. Confession and communion, which usually had been associated with a practicing Catholic's Easter duty in pre-famine Ireland, now became much more frequent. To encourage the laity, missions were held in nearly every parish in Ireland in the decade of the fifties. Pastoral gains thus made were consolidated by the introduction of a whole series of devotional exercises designed not only to encourage more frequent participation in the sacraments but to instill veneration by an appreciation of their ritual beauty and intrinsic mystery. The spiritual rewards, of course, for these devotional exercises were the various indulgences, which shortened either the sinner's or the sinner's loved one's time of torment in purgatory. The new devotions were mainly of Roman

<sup>32</sup> *Census of Ireland, 1871* (Dublin, 1875).



origin and included the rosary, forty hours, perpetual adoration, novenas, blessed altars, *Via Crucis*, benediction, vespers, devotion to the Sacred Heart and to the Immaculate Conception, jubilees, triduums, pilgrimages, shrines, processions, and retreats. These devotional exercises, moreover, were organized in order to communalize and regularize practice under a spiritual director and included sodalities, confraternities such as the various purgatorian societies, the Society of St. Vincent de Paul, and Peter's Pence as well as temperance and altar societies. These public exercises were also reinforced by the use of devotional tools and aids: beads, scapulars, medals, missals, prayer books, catechisms, holy pictures, and *Agnus Dei*, all blessed by priests who had recently acquired that privilege from Rome through the intercession of their bishops. Furthermore, this was the period when the whole world of the senses was explored in these devotional exercises, and especially in the Mass, through music, singing, candles, vestments, and incense.

THE EVIDENCE IN SUPPORT of this vast social change, of course, is too diversified and complicated to be presented coherently here. Still, in order to acquire perhaps at least the flavor of that phenomenon, if not its extent, it might be useful to follow selectively the career of the man who spearheaded that change in the initial years. "I was in Armagh," Cullen reported shortly after his arrival in Ireland to Tobias Kirby in Rome, "and saw nearly all the clergy." "The old Cathedral," he explained, obviously scandalized, "is awfully bad—the priests use only *one* tallow candle on the altars at mass in the Cathedral. Imagine what it must be elsewhere." Three months later Laurence Forde, Cullen's master of ceremonies at the Synod of Thurles, reported to Kirby that the opening day's solemnities were a grand Roman success with high mass being celebrated *alla* Palestrina, complete with assistant priest, deacon, and subdeacons. "I gave the seventh candlestick," Forde assured Kirby,

to Dr. Cullen at the Mass. I was not quite certain if it should be so, but I acted on your advice. It is not without its effect—I think it useful to go in some things even a little beyond strict practice for the sake of letting the bishops and clergy see the full solemnities of the Church.<sup>33</sup>

Some two years later, when he had been translated to Dublin, the richest and most influential diocese in Ireland, Cullen still was concerned about setting the proper tone. "We commenced the Jubilee here yesterday," he reported to Kirby in October 1852, "I sang more meo high mass attended by all the Chapter and dignitaries of Dublin." Mr. Faber, the celebrated English priest, "preached a magnificent sermon by far the most eloquent I ever heard," giving "a new tone and a Catholic one to Dublin." "We have

<sup>33</sup> May 21, Aug. 22, 1850, K.

the *40 ore*," he noted further, referring to his pro-Cathedral, "at Marlboro St. with great pomp and magnificence. The Church is ornamented with damask, a great machine erected and what is better crowds of people are attending. Deo Gratias. The devotion will be continued through the entire three months of the jubilee." He finally assured Kirby in Italian that he would soon see "the fruits of our preaching." Two months later, on December 8, in a letter headed "Evviva Maria," Cullen again reported to Kirby that the jubilee, the forty hours, and the novena in process were all very successful. "Here," he added even more significantly, "we are trying to enrol a large missionary body before next summer to wipe out the proselytizers everywhere." "It is necessary to see," Cullen concluded prudently in Italian, "if it will be successful, and then I will write to Propaganda. The Jesuits, Dominicans, Carmelites, Vincentians, Redemptorists, secular Priests will all join together—but."<sup>34</sup>

"The Jubilee," Cullen again assured Kirby less than two weeks later, on December 20, 1852, "has succeeded beyond all hope. All the churches are crowded with people trying to go to confession. Were the priests ten times as many as they are they could not hear them all." "I have done nothing lately," he added, breaking into Italian,

but to cure invalid marriages and remedy similar impediments. We must beg of the Pope to give a Jubilee of one month next May. It will put down all heresies—and set things right. . . . The priests are greatly fatigued with the Jubilee otherwise I w<sup>d</sup> apply at present to have it prolonged.

"Some of the people here," Cullen then noted, indicating that devotional practices were becoming contagious, "are anxious I should establish in the schools and convents the 'Child of Mary' such as they have in Waterford convent. Will you get me faculties to do this, and to establish every other sodality. I w<sup>d</sup> require to know what the indulgences are and what the rules."<sup>35</sup>

Early in the new year Cullen again wrote Kirby complaining that all was not well in the various parts of the Irish Church. "I wrote some days ago to Propaganda regarding the diocese of Ardagh," he explained in Italian in January 1853. "It seems to me," Cullen noted, "that it would be well to appoint a bishop quickly." "Father John Kilduff," he suggested, referring to a Vincentian on the Dublin mission, "a native of the diocese would be the saving of it." "He is a good preacher," Cullen continued, giving an interesting example of what he thought was important in a bishop, "a good theologian, full of zeal, and yet courageous enough. Such a man would be required to reform the diocese. He is about 33 years of age." "In the diocese of Cashel," Cullen then informed Kirby, turning to another trouble spot, "there is a parish called Doon, where I have heard

<sup>34</sup> Oct. 9, Dec. 8, 1852, K.

<sup>35</sup> Dec. 20, 1852, K.

there are seven or eight hundred apostates." "The Archbishop of Cashel, however," he reported, "does not want any noise made about it. Father Dowley, Superior of the Vincentians recently offered to give a mission but so far the offer has not been acknowledged." "The poor Archbishop," he added, "is very timid, and believes that he is always on the verge of death, even though he is in good health." "He is almost the only bishop," Cullen complained, "that has done nothing about what was prescribed in the Synod of Thurles." "Baptisms and confessions remain as they formerly were, and they also celebrate marriages in private houses. In almost all the other dioceses something at least has been done." "In this diocese of Dublin," he then explained,

all marriages and baptisms are celebrated in the churches. In the city and in the towns all the confessions are heard in the churches. In all the mountainous places where there are no churches nearby, if the distance is not too great, I told the priest to find every means of transporting the people to those distant churches—but if that were not possible to hear the confessions in private houses (except in case of illness), if the church is not more than two miles away.<sup>36</sup>

"Evviva S. Patrizio," Cullen greeted Kirby again some two months later on March 17, 1853, and continued in Italian, "I have already convened a provincial synod to be held in Dublin at Pentecost. The Bishops are not pleased." "Now it is necessary to see," Cullen explained, "quid agendum. There are things enough to be done—but it is difficult to put them in order and I must do all myself. There is no one who knows how to draw up a decree or write a line of Latin." "Monsignor Dixon," Cullen then reported, referring to the new archbishop of Armagh, "has already visited Ardagh and has promised to write in favor of Kilduff." It is Kilduff, Cullen added tenaciously in conclusion, who would be "the salvation of that unfortunate diocese."<sup>37</sup> Kilduff "will be consecrated here by me on S. Peters day." Dr. Dawson, the popular candidate among the Ardagh priests, "has written him a most foolishly impolite letter, which shows he was never fit to be a Bishop." Every attempt had been made "to get up some agitation against Kilduff by Dawsons friends—protests I believe have been sent to Rome—but the people are delighted, and the greatest part of the clergy—the appointment was absolutely necessary." "I have been told," Cullen added, referring to the archbishop of Tuam and the bishop of Clogher, "that Dr. McHale and Dr. McNally spoke against it—but this is only a report." "There is no doubt however," he assured Kirby again, "that K. [Kilduff] will be a blessing to the diocese—tho' he will have to carry his cross with the opp. of Dawson & Co." "I send you the acts and decrees of the Council in a day or two with a letter to the Pope," Cullen then noted, referring to his recently concluded provincial synod. "In Cashel," he then complained again of the archbishop, "I hear,

<sup>36</sup> Jan. 28, 1853, K.

<sup>37</sup> Mar. 17, 1853, K.

Dr. Slattery has not made a single change as yet. Marriages, baptisms, confessions still, as formerly in private houses. The same in several dioceses and I believe in Tuam." "It will be necessary," he advised darkly, "to do something in these matters." "But," he concluded characteristically, and appropriately breaking into Italian, "little by little all will be accomplished."<sup>38</sup>

This brief account of Cullen's early attempt at reforming the Irish Church in the interests of making and consolidating a devotional revolution is useful as a model, though a somewhat imperfect one, of his own continuing attempts, and those of his protégés, at reform over the next twenty-five years. In general Cullen preferred to promote men like Kilduff who were made in his own image and likeness. They were not only good preachers, adequate theologians, zealous, courageous enough, and young, but they were also generally strangers to the diocese and, therefore, they did not have any of the personal ties or loyalties that might inhibit them in their zeal for reform. If they were not recruited from the regular clergy, moreover, the new bishops were usually rectors or vice-rectors of seminaries—strict, stern, austere men who had both the experience of, and a proven talent for, efficient administration. They were also well aware that the new discipline they represented would not be popular among their priests, but if these bishops were ever to make their wills effective with their clergy, the bishops would have to depend on their patron's continued exertions on their behalf at Rome. They all tended, therefore, to be ultramontanes, because Rome was not only the theoretical but the actual source of their own and Cullen's real power in the Irish Church.

WHILE ALL OF THE FOREGOING may tell one something about what this devotional revolution consisted of and, at least partially, how it was made, the crucial question still remains—why did the Irish people respond so readily to the reform of their Church and become virtually practicing Catholics within a generation? The Great Famine was truly a gigantic psychological shock, and it certainly would be both neat and convenient to be able to assign so impressive a cause for so remarkable an effect. A guilt-ridden and frightened people turning more formally and fervently to their God in their hour of need makes more, indeed, than a good deal of superficial sense. The problem, of course, is that the devotional need appears to have been increasingly present before the famine, and only the adverse circumstances of population growth and the lack of money and personnel on the part of the Church prevented that need from being realized. The famine, therefore, was as much the occasion for as it was a cause of the devotional revolution being made and consolidated in Ireland, and one must probe more deeply if one is to understand why as well as how this remarkable historical phenomenon took place.

<sup>38</sup> [June 1853], K.

What I would like to suggest is that the devotional revolution which took place after the famine satisfied more than the negative factors of guilt and fear induced by that great catastrophe. There may indeed be something worse than the simple fear of being destroyed—the mounting terror in the growing awareness that one is being destroyed. The Irish, after all, had been gradually losing their language, their culture, and their way of life for nearly a hundred years before the famine. Education, business, politics, and communication in the written word, even more than in the spoken word, were all increasingly geared to English as the Irish were being effectively Anglicized, or, perhaps more appropriately, West Britonized. There has been so much concern, for example, in the study of Irish history in the nineteenth century with the geography of emigration that it has hardly been noticed that the Irish before the famine had nearly all become cultural emigrants, that they had in fact moved in their minds before a good many of them had actually to move in space.<sup>39</sup> In a word, then, Irishmen who were aware of being Irish were losing their identity, and this accounts in large part for their becoming practicing Catholics. The devotional revolution, I would argue further, provided the Irish with a substitute symbolic language and offered them a new cultural heritage with which they could identify and be identified and through which they could identify with one another. This is why, for example, Irish and Catholic have become almost interchangeable terms in Ireland, despite the attempts of Nationalists to make Irish rather than Catholic the inclusive term. “Take an average Irishman,” the celebrated Irish Dominican preacher, Father Tom Burke, said in 1872, “—I don’t care where you find him—and you will find that the very first principle in his mind is, ‘I am not an Englishman, because I am a Catholic! Take an Irishman wherever he is found, all over the earth, and any casual observer will at once come to the conclusion, ‘Oh; he is an Irishman, he is a Catholic!’ The two go together.”<sup>40</sup>

<sup>39</sup> Francis O’Neill to Kirby, Plymouth, July 13, 1853. K. O’Neill was a native of Waterford, but when he was ordained after his studies in the Irish College in Rome he had to go on the English mission, because there was no place available for him in the diocese of Waterford. Though the following passage is both interesting and illustrative in many ways, the important point is that the Irish speakers in Plymouth wanted to remain psychologically whole in revealing the secrets of their hearts. “Please God I hope to have a place in Waterford next year; as one can effect more where there are good hearts. Here the English Catholics never look at an Irish priest, but we have the poor Paddies who are the support of this mission and without whom the Bishop, would have to leave this place. The Irish here are about three or four hundred. Some of them cannot be got to make their confessions in English, at this the Bishop is most indignant. He says that it is pride, and also because they make more of the priest than the Sacrament. He has to keep a priest who can speak Irish. I cannot put two words of it together so in that point I am no help. Were I sure of having this good Bishop always I would not think of returning to Ireland but as this is not at all likely especially as he is an old man I shall get my Exeat. He finds many faults with the Irish but they are the ones that we cannot deny. He speaks frequently about the way the priests in Ireland *demand* money for the Sacraments. This is no false charge against them for all whoever had any experience of the system even in our own Diocese and also of it even since the Thurles Synod will acknowledge that too much cannot be said against it.”

<sup>40</sup> Thomas H. Burke, O.P., “The Supernatural Life of the Irish People,” in *Lectures on Faith and Fatherland* (London, n.d.), 117. Father Burke lectured extensively in the United States in 1872.

Finally it is necessary to observe that the making and consolidating of this devotional revolution had a wider and deeper significance than even making practicing Catholics of the Irish people in a generation. One can argue that the cause and effect relationship between what may be popularly called in the best current sociological jargon a group "identity crisis," and the resolution of it in what was fundamentally a religious revival has some very serious implications for, as well as allowing for some very interesting insights into, the history of the Irish people both at home and abroad in the nineteenth century. Daniel O'Connell, for example, if viewed as the bridge between the old and the new Ireland rather than as the divide between Old and Young Ireland, becomes not only more important but more understandable as the great transitional figure in modern Irish history.<sup>41</sup> Further, the devotional revolution and its general and particular causes are crucial to understanding the development of Irish nationalism and the cultural importance of Irish Catholicism in that development.<sup>42</sup> Moreover, the cultural revivals of Young Ireland in the middle of the century and the Gaelic movement at the end appear less ludicrous in the light of the identity crisis that had been taking place since the turn of the century, and Daniel Corkery and the other archpriests of the language movement in more recent times may indeed yet come into their own.<sup>43</sup>

<sup>41</sup> Sean O'Faolain, *King of the Beggars* (London, 1938), 367-68. In summing up the significance of O'Connell, O'Faolain wrote, "In whatever way one might try to define the ideal life of the Irish people, his image is likely to rise before the mind—always remembering that he came at the beginning and was only following his instinct in a groping use of the material to his hand. Lecky said that he studied men, not books; in studying men he found himself, and in finding himself he presented to his people a mirror of their reality. He is interesting in a hundred ways, but in no way more interesting than in this—that he was the greatest of all Irish realists who knew that if he could but once define, he would thereby create. He did define, and he did create. He thought a democracy and it rose. He defined himself, and his people became him. He imagined a future and the road appeared. He left his successors nothing to do but to follow him. They have added precision to his definition, but his definition is not altered; they have added to his methods, but his methods remain. You may break gold but it is gold still, fashion wood but it is wood still. The content of Irish life is the content of the Irish character, the dregs and the lees and the pure wine of this one man's recipe—to be purified indeed, to grow more rich in the wood with time, but never to lose the flavour of his reality, the composition of his mind."

<sup>42</sup> The author is at present engaged in writing the third volume—"The Devotional Revolution in Ireland, 1850-1878"—of his projected *History of the Roman Catholic Church in Ireland in the Nineteenth Century, 1780-1918*, in which he hopes to deal *in extenso* with this crucial relationship between nationalism and Catholicism as well as with many of the other themes only touched upon in this article.

<sup>43</sup> "The truth is, the Gaelic people of that century were not a mob, as every picture given of them, whether by historian or novelist, would lead one to think. They were mob-like in externals; and one forgives the historians if those externals threw them out, but how forgive the novelists? If not a mob what then were they? They were the residuary legatees of a civilisation that was more than a thousand years old. And this they knew; it was indeed the very pivot of all they did know, and the insult that followed on their poverty wounded them not only as human beings but as 'Children of Kings, Sons of Milesius!' ('Clanna righthe maca Mileadh'). With that civilisation they were still in living contact, acquainted with its history; and such of its forms as had not become quite impossible in their way of life, they still piously practised, gradually changing the old moulds into new shapes, and, whether new or old, filling them with a content that was all of the passing day and their own fields. What of art they did create in their cabins is poor and meagre if compared with what their fathers had created in the Duns of Kings and Grianans of Queens; yet the hem matches the garment and the clasp the book. Here hinted, then, what these historians scanted; and scanting the soul and the spirit of a people, what of that people have they profitably to speak? But history has belied the historians,



Last but not least in this necessarily less than complete catalog of what was significant in the devotional revolution is its importance for understanding the great Diaspora of the Irish people in the nineteenth century, as more than four million of them found new homes in a new world. Most of the two million Irish who emigrated between 1847 and 1860 were part of the pre-famine generation of nonpracticing Catholics, if indeed they were Catholics at all. They congregated in the ghettos of English, American, and Canadian cities where they acquired a fearful reputation for ignorance, drunkenness, vice, and violence. What the famine Irish actually represented, therefore, was a culture of poverty that had been in the making in Ireland since the late eighteenth century because of the pressure of population on the means of subsistence. That culture produced all the circumstances and most of the values that the British and the Americans were to find most repugnant in the Irish. The crucial point here, however, is that after the famine that culture of poverty was broken up in Ireland by emigration, and the new circumstances created by that breakup allowed for the emergence of other values.

Of the four million Irish, for example, who immigrated to the United States between 1845 and 1900, some 2,300,000 came after 1860. By 1860 there already were in Ireland 3,000 priests and 2,600 nuns for a Catholic population of 4,500,000, or one priest for every 1,500 people and one nun for every 1,700. In 1900 there were 3,700 priests and 8,000 nuns for a further reduced Catholic population of 3,300,000, or a ratio of one priest for every 900 and one nun for about every 400 people.<sup>44</sup> Besides this remarkable improvement in the clerical population vis-à-vis the Catholic population in Ireland, the Irish Church during this period exported a very large number of priests and nuns to help staff churches in the United States and the rest of the English-speaking world.<sup>45</sup> What these crude figures suggest is that the Irish were transformed as a people—men and women alike—into practicing Catholics. The succeeding waves of these recently created

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for that people, if they were but a mob, had died, and their nationality died with them: instead of which that nationality is vigorous today, not only at home, but in many lands abroad—'translated, passed from the grave.'" Daniel Corkery, *The Hidden Ireland: A Study of Gaelic Munster in the Eighteenth Century* (Dublin, 1925), 28–29.

<sup>44</sup> *Census of Ireland, 1861* (Dublin, 1863); *Census of Ireland, 1901* (Dublin, 1903).

<sup>45</sup> "Ireland has not only done a great deal during the past century for the Propagation of the Faith so that her missionaries and nuns and faithful people are now to be found foremost in everything good going on in the various missions of the old and new world but at present the whole country may be regarded as a vast recruiting field for sustaining these distant missions. We have in Kilkenny at present (to mention one instance) an American Christian Brother seeking for young men to join that order in the United States. They have at present 500 Irishmen among the Xtian Brothers of the United States and only 200 of all other nationalities. They have seven Colleges in the United States and the Superiors of them all are Irish. Nevertheless they are called the 'French Xtian Brothers.' At present they desire at least 50 more Irish postulants, as they find that none labour so zealously and efficiently in the American schools, as the Irish Brothers. The Brother has been only a few days in Ireland still he has already got twenty postulants. We have also Nuns of the Holy Cross who is seeking postulants in like manner. She told me that when she was over here two years ago she succeeded in getting twenty-five young ladies for her order in the United States, and that they all persevered. She now desires 25 more and has come over to search for them." Patrick Francis Moran to Kirby, Apr. 28, 1876, K.

devotional Catholics brought their cultural and religious needs and corresponding values with them when they emigrated, and in doing so they helped to reclaim those lapsed and nonpracticing "shanty" Irish. The newer, "lace-curtain" Irish found it progressively easier to assimilate to their new environment, because they were objectively less objectionable.

In a word, then, the Irish immigrants in this country in 1900 were a vast improvement over the generation of famine Irish who had arrived before 1860, and that improvement was not evident simply in terms of social behavior. Not only were later immigrants less drunken and less prone to violence, they also had acquired basic educational skills and were actually less poor. Average daily attendance in the Irish National System of Education increased from 100,000 in 1840 to nearly 500,000 in 1900, and this in spite of the fact that the population had been reduced by one-half over that period of time.<sup>46</sup> The literacy figures reveal that in 1861 45.8 per cent of Roman Catholics were unable to read and write, but by 1901 the figure had dropped to 16.4 per cent, a decline that was reflected in comparative literacy figures for various immigrant groups in the United States after the turn of the century.<sup>47</sup> Economic conditions in Ireland, furthermore, improved between 1840 and 1900, though the economy was certainly a very sick one, and the culture of poverty that was broken at the famine was liquidated partly by that improvement and partly by the continuing emigration, leaving the remaining population relatively less poor. No factor, then, was more important in the moral and social improvement of the Irish people either at home or abroad in the nineteenth century than the devotional revolution between 1850 and 1875; yet no aspect of recent Irish history has received less attention.

<sup>46</sup> Donald H. Akenson, *The Irish Education Experiment: The National System of Education in the Nineteenth Century* (London, 1970), 140, 346. There are no average daily attendance figures before 1852. In 1852 the number of children on the rolls was 544,604, and average daily attendance was 282,575; the number of children on the rolls in 1840 had been 232,560. I have, therefore, assuming there was improvement, calculated average daily attendance at something less than half of 232,560.

<sup>47</sup> *Ibid.*, 377. These percentages, of course, are for that portion of the population over five years of age. See also William D. P. Bliss, ed., *The New Encyclopedia of Social Reform* (New York, 1908), 598. "The total illiteracy of immigrants [to the United States] over fourteen years of age, in 1905, was 26.2 per cent. The females are, in general, more illiterate than the males." The study then cites figures for "the illiteracy of the races contributing more than 2,000 immigrants . . . for the same year." The statistics, presented under the heading "Northern and Western Europe (Chiefly Teutonic and Celtic)," are as follows: Scandinavian 0.6; Scotch 0.7; English 1.3; Bohemian and Moravian 1.7; Finnish 1.8; French 2.7; Irish 3.8; German 4.2; Dutch and Flemish 5.3; Italian (North) 14.0; Average 3.7.

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## The Vital Center, the Fair Deal, and the Quest for a Liberal Political Economy

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ALONZO L. HAMBY

"EVERY SEGMENT of our population and every individual has a right to expect from our Government a fair deal," declared Harry S. Truman in early 1949. In 1945 and 1946 the Truman administration had almost crumbled under the stresses of postwar reconversion; in 1947 and 1948 it had fought a frustrating, if politically rewarding, battle with the Republican Eightieth Congress. Buoyed by his remarkable victory of 1948 and given Democratic majorities in both houses of Congress, Truman hoped to achieve an impressive record of domestic reform.<sup>1</sup> The president systematized his past proposals, added some new ones, and gave his program a name that would both connect his administration with the legacy of the New Deal and give it a distinct identity. The Fair Deal, while based solidly upon the New Deal tradition, differed from its predecessor in significant aspects of mood and detail. It reflected not only Truman's own aspirations but also a style of liberalism that had begun to move beyond the New Deal during World War II and had come to maturity during the early years of the cold war—"the vital center."

THROUGHOUT THE HISTORY of the United States the main stream of reform has been within the broad Lockean-capitalist consensus to which most Americans subscribe. The Great Depression, however, had caused liberal reformers to question capitalism as never before; mass unemployment at home and the rise of an aggressive fascism out of the ruins of capitalism abroad seemed to provide proof that the old system had failed beyond repair. One logical response with appeal to many reform thinkers and leaders

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<sup>1</sup> *Public Papers of the Presidents: Harry S. Truman, 1949* (hereafter *Public Papers, 1949*) (Washington, 1964), 7. The best guide to the political history of the Truman era is Richard E. Neustadt, "Congress and the Fair Deal: A Legislative Balance Sheet," *Public Policy*, 5 (1954): 349-81.

was the movement for a popular front of all reform and radical forces, most strongly united by a determination to stop the spread of fascism but also seeking newer and better socioeconomic arrangements, even "revolutionary" ones. The New Deal itself, faced with the actual responsibility of governing, took a far more moderate course, searching for a viable middle way that would preserve capitalism; yet even the New Dealers, unable to overcome the depression, were increasingly driven to the conclusion that capitalism had become incapable of the growth needed to provide reasonably full employment.

The thirties did not exactly constitute the fabled "Red Decade" of right-wing mythologists. Most liberals who worked within the government sought American solutions to American problems and appear to have been only marginally influenced by foreign examples. Those outside the government were more likely to look toward European patterns. The most enduring appeal they found was in Scandinavian welfarism, but many were at least provisionally drawn to Soviet communism. A liberal of the thirties, quite in line with the popular-front mood, was more likely to think of himself as part of an undifferentiated Left and more prone to consider substitutes for capitalism than were earlier progressives. The failure of capitalism at home and abroad did not throw the liberals en masse into the Communist party, but it shook old assumptions to an extent that left few unaffected.<sup>2</sup>

Temporarily shattered by the Nazi-Soviet Pact of 1939, popular-front foreign policy staged a resurgence during World War II and received an aura of legitimacy from President Roosevelt's effort to forge a lasting alliance with the Soviet Union. During the war and the years immediately following, advocates of Soviet-American friendship could use the Roosevelt name and symbolism as a potent appeal. Yet at the same time World War II eroded the domestic side of popular frontism. The war eliminated the depression—as the New Deal had not—and demonstrated the potential of American industry. To a large extent, moreover, businessmen managed the economic war effort, and, while the liberals frequently criticized them on matters of detail, it was hard to refute the statistics of success. One result was a widespread repudiation of the psychology of scarcity, which had grown out of the long years of the depression. Leading progressives popularized the vision of an ever-expanding capitalist economy balanced by Keynesian fiscal methods and buffered by extensive social welfare programs. Their intellectual leader was the eminent economist Alvin H. Hansen and their political leader was Vice-President Henry A. Wallace, who demonstrated that it had become possible, even natural, to be a popular fronter

<sup>2</sup> Frank A. Warren, III, *Liberals and Communism: The "Red Decade" Revisited* (Bloomington, 1966), is a sober and intelligent analysis of popular frontism in the 1930s. William E. Leuchtenburg mentions the appeal of Scandinavia in *Franklin D. Roosevelt and the New Deal* (New York, 1963), 345.

in foreign policy and an advocate of "progressive capitalism" at home. The liberal mission was no longer to achieve a new socioeconomic system or even to prop up a "mature," worn-out economy; it was to realize capitalism's capacity for endless growth.<sup>3</sup>

The cold war completed the demise of the popular-front mood. Groups and individuals that thought of themselves as liberal came increasingly to perceive the Soviet Union as an expansionist, totalitarian force and the American Communist party as the slavish, antiliberal representative of Soviet despotism. In 1947 an influential group of liberals established Americans for Democratic Action (ADA) with the express purpose of isolating Communists and pro-Communists from the main stream of liberal politics. Key foreign policy events that followed—the Russian rejection of the Marshall Plan, the Czech coup, and the Berlin Blockade—inclined most progressives toward the ADA position. In 1948 the *New Republic*, probably the most sensitive barometer of progressive opinion, rejected the popular-front style of Henry Wallace's presidential candidacy and endorsed Truman. Wallace's weak showing on election day demonstrated a massive liberal repudiation of the Soviet Union and the Communist party.<sup>4</sup>

By 1949 the ADA was the dominant progressive organization, and signs of a transformation were appearing throughout the liberal community. The editor of the *New York Post*, T. O. Thackrey, had endorsed Wallace for president. In April 1949 the paper's owner and publisher—his wife, Dorothy Schiff Thackrey—fired him. It is significant that Mrs. Thackrey defined the break in terms of attitudes toward communism, arguing that the Communists posed "new threats to democracy" and asserting that henceforth the paper would fight with equal vigor "all totalitarianism, whether Fascist or Communist." Her new editor, James A. Wechsler, was an ADA leader and a militant anti-Communist, who quickly dismissed popular-front columnists.<sup>5</sup>

The *New Republic* continued the shift it had begun in 1948. Its publisher, Michael Straight, who had once regarded Henry Wallace as a personal hero, undertook a speaking tour on behalf of the ADA and delighted its leaders by bringing his magazine into nearly total agreement with the organization's viewpoint. The Italian-American intellectual, Max Ascoli, established the *Reporter* as a new outlet for moderate, tough-minded, anti-Communist liberalism. The Congress of Industrial Organizations expelled

<sup>3</sup> I have discussed these themes in "Sixty Million Jobs and the People's Revolution: The Liberals, the New Deal, and World War II," *Historian*, 30 (1968): 578–98; and in "The Liberals, Truman, and FDR as Symbol and Myth," *Journal of American History*, 56 (1970): 859–67. Ironically the Communist party itself contributed to the decline of domestic popular frontism by pursuing a wartime national unity line. See Irving Howe and Lewis Coser, *The American Communist Party: A Critical History* (2d ed.; New York, 1962), ch. 9.

<sup>4</sup> Alonzo L. Hamby, "Henry A. Wallace, the Liberals, and Soviet-American Relations," *Review of Politics*, 30 (1968): 153–69; "1948: The New Beginning," *New Republic*, Sept. 27, 1948, p. 32.

<sup>5</sup> *New York Post*, Apr. 6, 1949; James A. Wechsler, *The Age of Suspicion* (New York, 1953), 238–41.

popular-front unions. "We in American labor will fight totalitarianism from the right or the left," declared Philip Murray, the president of the CIO, "We regard the human welfare state as America's middle way."<sup>6</sup>

Murray's remark epitomized a new mood that conceived of liberalism as a center doctrine midway between the totalitarian poles of fascism and communism. Implicit in the new self-image was a slight moderation, a decline of utopian hopes and aspirations, a somewhat stronger suspicion of big government, and increasing doubts about the goodness of human nature. It was no coincidence that four significant books expressing this viewpoint in one manner or another appeared in 1949.

In *Target: You*, Leland Stowe, a widely read foreign correspondent, addressed himself to "Mr. American Middle Man"—the target of fascist and Communist totalitarianism, of monopolistic "Big Capitalism" and Communist Marxism—and argued that the future of American democracy depended upon the maintenance of a "strong political Center" that would counter both domestic communism and right-wing extremism by providing economic security and securing civil liberty under a rule of law.<sup>7</sup>

Max Ascoli in *The Power of Freedom* depicted the earth as caught up in a worldwide civil war with one side struggling to maintain freedom by finding the middle way between unrestrained capitalism and total socialization, the other attempting to achieve "the total subjection of men on a world-wide scale." Stressing the limitations of human nature and the unattainability of utopias, Ascoli unabashedly admitted that he was a disciple of such thinkers as Edmund Burke, Alexander Hamilton, and Alexis de Tocqueville. Yet he found no inconsistency in declaring: "I am a liberal, and I don't want to add any qualifying adjectives."<sup>8</sup>

In *Strategy for Liberals* the political journalist Irwin Ross used militant rhetoric and projected an ambitious reform program. Yet he carefully typed his ideal polity as the "Mixed Economy" and distinguished it not simply from fascism and communism but also from socialism, which, with its complete control of industry, detailed planning, massive bureaucracy, and control of communications, contained within itself "if not the seeds of decay, certainly the seeds of totalitarianism."<sup>9</sup>

Arthur M. Schlesinger, Jr. gave the new liberalism a name with the publication of *The Vital Center*. An exercise in political philosophy and an exhortation to American progressives, the volume won an impressive reception. "It seemed to me one of those books which may suddenly and

<sup>6</sup> James Loeb, Jr. to Edith Fountain, Feb. 2, 1949, Americans for Democratic Action Papers (hereafter ADA Papers), State Historical Society of Wisconsin, Madison, Wis.; M. A. [Max Ascoli], "What We Stand For," *Reporter*, Dec. 20, 1949, p. 2; "Our Liberalism," *ibid.*, Mar. 28, 1950, p. 1; Max M. Kampelman, *The Communist Party vs. the C.I.O.* (New York, 1957), 256, and chs. 11–13; David J. Saposs, *Communism in American Unions* (New York, 1959), chs. 17–20.

<sup>7</sup> Leland Stowe, *Target: You* (New York, 1949), 164, *passim*.

<sup>8</sup> Max Ascoli, *The Power of Freedom* (New York, 1949), xiii, 8, 70–71, *passim*.

<sup>9</sup> Irwin Ross, *Strategy for Liberals* (New York, 1949), 45, *passim*.



clearly announce the spirit of an age to itself," wrote Jonathan Daniels. Deeply influenced personally and intellectually by Reinhold Niebuhr, Schlesinger castigated the popular-front liberals as sentimental believers in progress and human perfectionism who, yearning for utopias, had been seduced by the surface idealism of communism and the Soviet experiment. Awake only to the evils of fascism, they had sympathized with at least some aspects of the Soviet experience and had accepted the Communists as allies in a common struggle, not understanding that such a tactic could lead only to self-destruction. The "restoration of radical nerve" had come with the rise of a non-Communist Left in Europe and the United States, largely through the efforts of younger liberals whose impressions of the Soviet Union stemmed from the Stalinist purges of the 1930s rather than the idealism of the Russian Revolution. The new liberalism—or "radicalism" as Schlesinger preferred to call it—unconditionally rejected all varieties of totalitarianism. Applied to foreign affairs it stood for a dual policy of vigilantly containing communism and encouraging the democratic Left abroad. Believing "in the integrity of the individual, in the limited state, in due process of law, in empiricism and gradualism," it was acutely aware of the weaknesses of human nature and of the dangers of excessive concentration of power. Devoted to the furtherance of individual liberty, it stood for a mixed economy, featuring partial government planning and ownership, antitrust action to discipline private big business, and welfare programs to provide a minimum of security and subsistence to all. The conception of liberalism as a sort of centrism had its liabilities. Schlesinger found it natural to identify with "responsible conservatives" such as Charles Evans Hughes and Henry L. Stimson; liberals, he suggested, might find common cause with this group, especially on matters of civil rights and civil liberties. Doubtless he was correct, and it was tempting, after militantly rejecting the revolutionary totalitarian ideology of communism, to conceive of the liberal effort to preserve humane, democratic values as akin to an intelligent conservatism; yet even the creed of a Hughes or a Stimson provided few answers for the problems that preoccupied the liberals. Unfortunately it was but a short step from the vital center to the superficialities of the "New Conservatism" of the 1950s.<sup>10</sup>

Whatever its inner weaknesses, the vital-center approach gave the liberal movement a moral integrity and consistency that had been absent during the popular-front era. Its implications, moreover, went beyond the affairs of diplomacy or the tactical wisdom of a liberal-Communist alliance: its approach to political economy rejected what remained of domestic popular frontism and idealized the New Deal as an effort to establish a mixed economy that would preserve the essentials of capitalism while mitigating its abuses. Even the business community was recognized as a potentially

<sup>10</sup> Arthur M. Schlesinger, Jr., *The Vital Center* (Boston, 1949), 208–09, 223–24, *passim*; Jonathan Daniels, "Ready to Be Radical," *Saturday Review of Literature*, Sept. 10, 1949, pp. 11–12.

constructive, if frequently wrongheaded, force in American life. The vital-center liberals looked to Niebuhr for a sociopolitical theory and to Keynes for an approach to economics, convinced that this combination provided the best possible foundation for human freedom. In 1948 a group of Keynesians published the major liberal economic manifesto of the Truman era, *Saving American Capitalism*.<sup>11</sup> The title accurately represented the way in which vital-center liberalism was a return to the traditional American progressive impulse.

The legislative goals Truman announced for his administration, while not devised to meet the needs of an abstract theory, were well in tune with the vital-center approach: anti-inflation measures, a more progressive tax structure, repeal of the Taft-Hartley Act, a higher minimum wage, a farm program based on the concepts of abundant production and parity income, resource development and public power programs, expansion of social security, national medical insurance, federal aid to education, extensive housing legislation, and civil rights bills. The president's most controversial request was for authority to increase plant facilities in such basic industries as steel, preferably through federal financing of private enterprise but through outright government construction if necessary. Roundly condemned by right-wing opponents as "socialistic" and soon dropped by the administration, the proposal was actually intended to meet the demands of a prosperous, growing capitalist economy and emerged from the Fair Deal's search for the proper degree of government intervention to preserve the established American economic structure. "Between the reactionaries of the extreme left with their talk about revolution and class warfare, and the reactionaries of the extreme right with their hysterical cries of bankruptcy and despair, lies the way of progress," Truman declared in November 1949.<sup>12</sup>

The Fair Deal was a conscious effort to continue the purpose of the New Deal but not necessarily its methods. Not forced to meet the emergencies of economic depression, given a solid point of departure by their predecessors, and led by a president more prone than FDR to demand programmatic coherence, the Fair Dealers made a systematic effort to discover techniques that would be at once more equitable and more practical in alleviating the problems of unequal wealth and opportunity. Thinking in terms of abundance rather than scarcity, they attempted to adapt the New Deal tradition to postwar prosperity. Seeking to go beyond the New Deal while preserving its objectives, the Truman administration advocated a more sweeping and better-ordered reform agenda. Yet in the quest for political means, Truman and the vital-center liberals could only fall back upon one of the oldest dreams of American reform—the Jacksonian-Populist vision of a union of producing classes, an invincible farmer-

<sup>11</sup> Seymour Harris, ed., *Saving American Capitalism* (New York, 1948).

<sup>12</sup> *Public Papers, 1949*, 1-7, 552.

labor coalition. While superficially plausible, the Fair Deal's political strategy proved too weak to handle the burden thrust upon it.

The Fair Deal seemed to oscillate between militancy and moderation. New Dealers had frequently gloried in accusations of "liberalism" or "radicalism"; Fair Dealers tended to shrink from such labels. The New Dealers had often lusted for political combat; the Fair Dealers were generally more low keyed. Election campaigns demanded an aggressiveness that would arouse the Democratic presidential party, but the continued strength of the conservative coalition in Congress dictated accommodation in the post-election efforts to secure passage of legislative proposals. Such tactics reflected Truman's personal political experience and instincts, but they also developed naturally out of the climate of postwar America. The crisis of economic depression had produced one style of political rhetoric; the problems of prosperity and inflation brought forth another.

The Fair Deal mirrored Truman's policy preferences and approach to politics; it was no more the president's personal creation, however, than the New Deal had been Roosevelt's. Just as FDR's advisers had formulated much of the New Deal, a group of liberals developed much of the content and tactics of the Fair Deal. For the most part these were the men who had formed a liberal caucus within the administration in early 1947 shortly after the Republican triumph in the congressional elections of 1946, had worked to sway the president toward the left in his policy recommendations and campaign tactics, and had played a significant, if not an all-embracing, role in Truman's victory in 1948. Truman's special counsel, Clark M. Clifford, was perhaps the most prominent member of the group, but Clifford, although a shrewd political analyst, a persuasive advocate, and an extremely valuable administrative chief of staff, was neither the caucus's organizer nor a creative liberal thinker. Others gave the Fair Deal its substance as a program descending from the New Deal yet distinct from it.<sup>13</sup>

The founder of the liberal caucus, Oscar R. Ewing, exemplified better than any other prominent member of the Truman administration the linkage between the New Deal and the Fair Deal. Even as a young man in turn-of-the-century Indiana he had possessed a consuming interest in Democratic politics and social welfare problems. At the age of sixteen he was secretary to the state Democratic committee, and for a time he planned to become a social worker. Instead, after graduating from the Harvard Law School, he settled in New York and pursued a highly successful practice as a partner of first the elder, then the younger, Charles Evans Hughes. By the 1940s he had also become one of the most prominent Democrats in the state and was frequently mentioned as a possible candidate for high

<sup>13</sup> Cabell Phillips, *The Truman Presidency: The History of a Triumphant Succession* (New York, 1966), 162-65; and Irwin Ross, *The Loneliest Campaign: The Truman Victory of 1948* (Signet ed.; New York, 1969), 27-29, discuss the liberal caucus; both probably overemphasize the importance of Clifford.

office. During Robert E. Hannegan's tenure as chairman of the Democratic National Committee (1944-47), Ewing was vice-chairman and, after Hannegan's health collapsed, acting chairman. Appointed administrator of the Federal Security Agency<sup>14</sup> in 1947, he began a drive to revitalize the agency and secure cabinet status for it. It was he who took the initiative in mobilizing the liberals within the Truman administration for the crucial struggles of 1947 and 1948.

Ewing's advocacy of comprehensive social welfare legislation—a popular magazine described him as “Mr. Welfare State himself”—was the end result of a tradition that had begun with the social workers of the Progressive era, had found partial realization during the New Deal, and was now struggling for complete fulfillment. Ewing also represented a type of Democrat who had developed during the New Deal—the staunch, partisan regular who was nevertheless committed to social welfare liberalism and identified his party with it. The strongest fighter within the administration for expanded welfare programs, he did not shrink from debate with the opposition. “It is the fate of the American liberal to be a scrapper,” he remarked. Accepting the Sidney Hillman Award from the Amalgamated Clothing Workers Union in March 1950, he defined the key to America's future as “the protection and extension of equal opportunity for all our people—opportunity to live, to advance, to think, to achieve.” Especially in 1949 and 1950 he engaged in lusty verbal combat with his conservative opponents—“the League of Frightened Men,” he called them. Ewing demonstrated the way in which the New Deal, and indeed the whole progressive social welfare tradition, provided a solid basis for the Fair Deal, but his ideas, although they went beyond New Deal welfare programs, did not give the administration its claim to a separate identity. His style, as it turned out, was not especially productive; someone doubtless had to speak out against the bitter-end opponents of social welfare reforms, but Ewing only exposed himself to defeat by doing so. His militant advocacy of national health insurance not only failed to put the proposal over; it led to a backlash and caused Congress to reject an administration reorganization plan that would have created a cabinet-level Department of Welfare with Ewing as its first secretary. His personal defeat on this issue exemplified many of the difficulties the Fair Deal encountered when it adopted the militant tones of years past.<sup>15</sup>

WHILE EWING REPRESENTED CONTINUITY, Leon H. Keyserling and Charles F. Brannan gave the Fair Deal much of its distinctive approach. Both men

<sup>14</sup> Established during the New Deal, the Federal Security Agency became one of the major components of the present-day Department of Health, Education, and Welfare.

<sup>15</sup> *New York Times*, June 2, Sept. 14, 1945; Feb. 3, Mar. 17, Apr. 13, 1946; Dec. 3, 1947; Jan. 28, 1948; Jan. 10, Feb. 19, May 20, Aug. 17, 1949; Ewing, speeches in the *Congressional Record*, 81st Cong., 2d sess., 1950, app., pp. A1844-45, A4071-73; *Current Biography*, 1948, 193-96; Monte M. Poen, “The Truman Administration and National Health Insurance” (Ph.D. dissertation, University of Missouri, 1967), 140-42, 145-50, 155-56, 176, 188-89; Arthur M. Schlesinger, Jr., *The Coming of the New Deal* (Boston, 1958), 554.

served their political apprenticeships during the New Deal, but both formulated important criticisms of it and sought new techniques to achieve the objectives of liberal reform.

Keyserling, educated at Columbia University—where he was influenced by Rexford G. Tugwell—and the Harvard Law School, had gone to Washington in the early days of the Roosevelt administration to work for Jerome Frank in the Agricultural Adjustment Administration. He attracted the attention of Senator Robert F. Wagner, who made him an administrative assistant; during the next several years he was a central figure in the drafting of some of the most important legislation of the 1930s, including the National Labor Relations Act. Subsequently he was general counsel of the U.S. Housing Authority, later the National Housing Agency. In 1944 he took second prize in a widely publicized contest on the achievement of postwar prosperity with an essay urging an expansion of the economy to provide jobs for all. In 1945 he was active in the struggle for full-employment legislation. With Senator Wagner's backing he was a natural choice for the new Council of Economic Advisers, established by the Employment Act of 1946.<sup>16</sup>

During 1947 and 1948 Keyserling was a valuable member of the administration liberal caucus. At the same time he was gaining a public reputation as the most imaginative and articulate economist in the government. When the bland and moderate Edwin Nourse resigned as chairman of the Council of Economic Advisers in October 1949, Keyserling was automatically the liberals' candidate for the post, and the ADA spearheaded an intensive lobbying campaign in his behalf. After a long delay, in the spring of 1950 the president gave Keyserling the appointment.<sup>17</sup>

Although he had won formal appointment as the chief economic spokesman of the administration and had long been valued by the most able members of the president's staff, Keyserling appeared rather insecure. Academic economists were cool toward him because he lacked the appropriate pedigree of university degrees, though in mobilizing support for his promotion, the ADA found most liberal economists willing to support him, not as the best man for the job, but simply as the best possible alternative. Within the administration he had to live down his reputation as an Ivy League liberal ideologue; he seized opportunities to remind listeners that he had been born in South Carolina and could produce a letter of commendation from Robert A. Taft. Perhaps such difficulties were responsible for his enormous vanity and stuffy manner. Yet his brilliant mind transformed Truman's style and aspirations into an economic program.<sup>18</sup>

<sup>16</sup> *Current Biography*, 1948, 352–55; author's interview with Keyserling, Sept. 20, 1967; J. Joseph Huthmacher, *Senator Robert F. Wagner and the Rise of Urban Liberalism* (New York, 1968), 147, 163–64, 190, 192, 211, 224–25, 296–97.

<sup>17</sup> Charles La Follette to Harry S. Truman, Oct. 20, 1949, and La Follette to William Green, Oct. 27, 1949, both in ADA Papers; "Toward Full Employment," *New Republic*, May 22, 1950, p. 8.

<sup>18</sup> Robert S. Allen and William V. Shannon, *The Truman Merry-Go-Round* (New York, 1950), 79–81.

In line with the mood of the Fair Deal, Keyserling assiduously avoided labels more specific than "forward-looking" or "the middle way" for his ideas. He noted on the first page of his essay in *Saving American Capitalism* that he rejected "classification within any 'school of thought' or endorsement of any 'general theme' or 'purpose' which this collection may be deemed by some to represent." Adamantly refusing to be typed as a Keynesian, he frequently criticized New Deal economics: "Neither those 'liberals' who betray nostalgia for the New Deal of the thirties which accomplished much but not nearly enough, nor those 'conservatives' who would reincarnate the brutal and reckless economic philosophy of the twenties should be allowed to say the last word."<sup>19</sup>

On the surface his ideas and advice seemed an odd mixture of liberalism and conservatism. Writing to Clark Clifford in December 1948 with suggestions for the State of the Union message, he sounded like a conservative: "I am particularly concerned about the discussion of the economic program, which seems to imply that the Government is going to do the whole job. . . . The first responsibility for employment and production rests with business." Yet a few days later he was advocating more ambitious public-housing schedules than those proposed by the National Housing Authority. He sounded like a conservative when he emphatically disclaimed responsibility for the controversial proposal to expand basic industrial plants. He sounded like a New Dealer when he urged delegates to the convention of the Meat Cutters Union to push for higher wages and when he declared that "accruals of fat earnings" justified such demands. Shortly thereafter, however, he was reassuring business that "nobody in Washington has ever taken the position that the American economy could expand without profits."<sup>20</sup>

Keyserling's critique of New Deal economics had several themes. First of all, the New Deal had failed to grasp the virtual impossibility of the task it had undertaken: the lifting of the nation out of the depression. Those who argued that the New Deal would have been successful with a more massive spending effort were probably wrong. Government alone simply could not solve great economic crises, and if the New Deal could not be blamed for its failure, the New Dealers could be blamed for not learning the lessons of that failure.

The New Dealers also had become too dogmatic in their adoption of the antitrust persuasion. "Today some industries which are organized on a large, integrated basis are charging prices under the limit of what the traffic will bear," Keyserling wrote in 1948. By contrast, home building, the

<sup>19</sup> Keyserling, "Deficiencies of Past Programs and Nature of New Needs," in Harris, *Saving American Capitalism*, 81-94.

<sup>20</sup> Leon H. Keyserling to Clifford, Dec. 20, 1948, Jan. 3, 1949, both in Clark M. Clifford Papers, Harry S. Truman Library, Independence, Mo.; *New York Times*, Jan. 19, Feb. 28, Apr. 21, 1949.



most fragmented industry in the country, "has been notoriously inefficient, highly resistant to technological change, and periodically prices its product out of the market." The antitrust laws should be used to prosecute monopolistic wrongdoing, but "we cannot re-create the pre-Civil War pattern." The liberals needed instead to ask if there were not instances in which monopolistic concentration might be "used to stabilize rather than to exploit the economy." Conversely, they needed to undertake a more searching analysis of the problems that competition presented to economic stability. Some degree of economic coordination, as voluntary as possible, would always be desirable and, during times of economic difficulty, essential.

The adoption of Keynesianism by the New Dealers had not provided American liberalism with an economic panacea, Keyserling argued. Keynesianism might be useful during a depression, but it raised more problems than it solved by its remedies for inflation. Higher taxes and interest rates bore most heavily upon the lower and lower-middle classes. Cutbacks in government spending meant the sacrifice of "national objectives which we should not forgo merely because we are prosperous."

Finally, he charged, the New Dealers had lost faith in the potential of capitalism. Considering the system pathologically unstable, even if for some reason worth saving, they awaited the inevitable onset of a major depression armed with vast government programs, which probably would be no more successful than the New Deal itself. They had failed to address themselves to the potential of the American economy; they had not formulated theories for the maintenance of prosperity. "The people of America need to be electrified by our limitless possibilities, not frightened into action by prophets of disaster."

American capitalism, as Keyserling envisioned it, had virtually unlimited opportunities for growth; an ever-expanding economy could produce undreamed-of abundance and material gain for all classes. The liberals should concentrate not on reslicing the economic pie but rather on enlarging it. Business could expect higher profits, labor better wages, farmers larger incomes, and, above all, those at the bottom of the economic scale could experience a truly decent life. The federal government should publicize these possibilities; it should provide education and guidance to the private forces whose responsible cooperation would be imperative. Keyserling recommended the initiation of a "National Prosperity Budget" in which the government would lay down targets for employment and production, indicate priority needs, and sketch out price and wage recommendations. It would be purely advisory, depending upon the cooperation of the private sectors for implementation.

The government would not be passive. It would continue to police the economy against monopolistic abuses, dictate minimum wages, use Keynesian fiscal and monetary techniques, and even impose selective controls if conditions demanded. It would provide important programs and services

—such as low-cost housing, social insurance, education, and resource development—that fell outside the realm of private enterprise. Washington, however, could not keep the economy growing by itself. Expansion demanded voluntary cooperation: “The widening of this area of voluntary cooperation, through common study of common problems, is the only way that our highly industrialized and integrated economy can steer between the danger of periodic collapse and the danger of excessive governmental centralization of power.”

To those who feared that expansion meant boom-and-bust inflation, Keyserling replied that the growth years 1927–29 had constituted an era of remarkable price stability. Economic policy should concentrate less on prices as such and more on the relationship between wages, prices, and profits; it should work for the optimum balance between consumer purchasing power and corporate income in order to maintain full employment and expansion. The New Dealers, he believed, had turned too frequently toward controls to fight inflation after World War II. Selective controls might be necessary at times, but the way to deal with inflation was to enlarge productive capacity to meet demand. Keyserling did not shrink from stimulative government spending in times of prosperity, and although he would not admit it, he was willing to trade a mild inflation for growth. Such an alternative was greatly preferable to the achievement of price stability via a “downward ‘correction’” or recession. Higher unemployment and lower production might keep prices stable but hardly contributed to the overall health of the economy. “The idea that we can protect production and employment by reducing them ‘a little bit,’” he warned, “is about as safe as the ancient remedy of blood-letting.”<sup>21</sup>

During the first half of 1949 Keyserling transformed his vision of abundance to solid figures. Assuming an annual growth rate of three per cent and constant dollar values, the gross national product could rise from \$262 billion in 1948 to \$350 billion in 1958, national income from \$226 billion to \$300 billion. In 1948 almost two-thirds of all American families had lived on incomes of less than \$4,000 a year; by 1958, \$4,000 could be the minimum for all families. It would require only about half of the GNP increase to attain this goal, leaving a substantial sum for government programs and the enhancement of private incomes at other levels. Poverty thus could be eliminated without a redistribution of wealth. Progressive reform did not necessarily mean social conflict; rather it required intelligent cooperation.<sup>22</sup>

<sup>21</sup> Keyserling, “Deficiencies of Past Programs,” 81–94; Keyserling, “For a National Prosperity Budget,” *New York Times Magazine*, Jan. 9, 1949, pp. 42–43, 45; Keyserling, “The Middle Way for America,” *Progressive*, May 1949, pp. 5–9; Keyserling, “A Policy for Full Employment,” *New Republic*, Oct. 24, 1949, pp. 13–15.

<sup>22</sup> Keyserling, address in San Francisco, Sept. 18, 1949, Clifford Papers; [Keyserling] “Memorandum Relating to \$4000 Minimum Standard of Living,” Sept. 30, 1949, George Elsey Papers, Truman Library; Keyserling, “Planning for a \$300 Billion Economy,” *New York Times Magazine*, June 18, 1950, pp. 9, 24–27.

Truman adopted Keyserling's figures and rhetoric. Speaking to a Kansas City audience in the fall of 1949, he acclaimed the nation's history of economic growth and increasingly higher standards of living and declared his determination to continue the process. He talked of the \$300 billion national income and the \$4,000 family minimum. "That is not a pipe dream," he asserted. "It can be done."<sup>23</sup>

Keyserling had not discovered the idea of economic growth, although his ego seemed at times to tempt him to imply that he had. The growth levels of World War II had awakened many economic thinkers—Alvin Hansen and Henry Wallace, for example. Nor was Keyserling fair in his assertions that the New Deal Keynesians really accepted the business cycle and that their remedies could not be put into effect until a depression had already hit the economy. The Keynesians sought at the least to smooth out the business cycle so that depressions would be eliminated altogether, at best to maintain a constant growth without even periodic recessions.

If some of Keyserling's polemics rested on artificial assertions, his broad conception of economic expansion was nevertheless inspiring and enormously constructive. The major difficulty in the program he advocated was his reliance on voluntarism, his faith in education, his belief that group conflict could be mitigated by alluring vistas that promised gains for all. In accord with the general approach of the Fair Deal, Keyserling sought to base his economics on a politics of consensus, which neither he nor other Fair Dealers were ever able to achieve. His dream of an ever-prosperous society based on voluntary cooperation was almost as utopian as the scheme of a nineteenth-century anarchist.<sup>24</sup> Fortunately there were surer, if less perfect, ways of promoting economic growth. Keyserling defined important goals for the administration and captured the ear of a president who respected his intellectual ability and liked him as an individual.

CHARLES F. BRANNAN, who was as much a product of the New Deal as Keyserling, had begun his career in Colorado politics as a disciple of the old progressive, Edward Costigan, and an associate of Oscar L. Chapman, a dedicated liberal whom Truman appointed secretary of the interior in 1949. During the Roosevelt era Brannan had worked as an attorney for the Resettlement Administration and had been a regional director of the Farm Security Administration. Long close to the neo-populist National Farmers Union, he was a personal friend of its president, James G. Patton. Moving to Washington as assistant secretary of agriculture in 1944, Brannan quickly established himself as a loyal and capable lieutenant. In 1948 he took command of the department with the blessings of the outgoing

<sup>23</sup> *Public Papers, 1949*, 494.

<sup>24</sup> Richard Lester, "Truman Economics—1950 Model," *New Republic*, Jan. 28, 1950, pp. 11-13.

secretary, the moderate Clinton Anderson, and the enthusiastic endorsement of the Farmers Union, which had bitterly fought Anderson. No man, not even the elder or the younger Henry Wallace, had entered the office of secretary of agriculture with clearer credentials as an aggressive liberal. Like Keyserling, Brannan used the concept of abundance as an intellectual foundation. The politicoeconomic strategy that he formulated constituted the Fair Deal's clearest break with the New Deal.

In the fall of 1948 Brannan's astute advice on political strategy and his vigorous campaigning won the attention of Truman and brought him into the White House inner circle. Almost alone Brannan grasped that Midwestern farmers were apprehensive about the future of price supports and that the Republican Eightieth Congress, by failing to enlarge government storage facilities, had practically guaranteed that grain prices would decline during the presidential campaign. Truman and his liberal advisers quickly adopted Brannan's counsel of attacking the GOP as the party of opposition to price supports, and the secretary himself carried the message into farm areas with a tirelessness that shamed other cabinet members. Truman's unexpected success in the rural Midwest made Brannan one of the major figures of the administration. It also suggested new political strategies to liberals both inside and outside the government.<sup>25</sup>

Many progressives believed that the farm results represented a new trend in liberal politics. To the influential columnist Samuel Grafton, 1948 had been "a year of deep and quiet decision" for farmers; the election indicated that they had overcome their conservative biases in favor of their practical need for government support and would turn increasingly to the Democratic party.<sup>26</sup> If such were the case, then the task of the liberals was to encourage and consolidate this trend. The ultimate result would be a new Democratic party with a more solidly liberal base than ever before, a liberalism that would fuse the outlook and voting power of labor with an apparently reborn Midwestern agrarian insurgency. The liberal cause would be greatly strengthened and the conservative forces proportionately weakened. Within the Republican party the number of Midwestern reactionaries would decline; within the Democratic party the Southern conservatives would have less leverage.

The first imperative was to establish lines of communication between the farmers and the liberal-labor forces. The ADA began the process by calling a conference of about thirty farm and labor leaders, which met in Chicago at the end of February 1949. The farm leaders included James Patton from the Farmers Union, Murray Lincoln and some other progressive dissenters from the Farm Bureau, Jerry Voorhis and others from the cooperative movement, and several local Grange officials. Among the labor delegates were representatives of the Railway Trainmen, the Textile

<sup>25</sup> Allen J. Matusow, *Farm Policies and Politics in the Truman Years* (Cambridge, Mass., 1967), 170-85; Allen and Shannon, *Truman Merry-Go-Round*, 114-16.

<sup>26</sup> Samuel Grafton in the Chicago *Sun-Times*, Jan. 26, 1949.

Workers, the International Ladies Garment Workers Union, and the United Auto Workers. The meetings amounted simply to an exchange of views; the conference made no effort to hammer out a legislative program or draft a call to action. Yet James Loeb, the ADA's executive secretary, found the sessions "an exciting experience." The discussions were friendly despite some disagreements, and many of the participants favored more conferences at the state and local levels. "The farm and labor groups are moving, slowly but definitely, in the direction of mutual understanding," declared Loeb. "The encouragement of this process can have a lasting effect on the future history of America."<sup>27</sup>

The administration took the next step in April with the introduction in Congress of a new farm program, which had been drawn up under Brannan's direction. The Brannan Plan was difficult and complex in detail, but essentially it was an effort to maintain farm income at the record high level of the war and immediate postwar periods while letting market prices fall to a natural supply-demand level. Brannan thus proposed to continue the New Deal policy of subsidizing the farmers, but he broke dramatically with the New Deal technique of restricting production and marketing in order to achieve artificially high prices.

Many agrarian progressives, including Henry A. Wallace himself, had long been troubled by the price-support mechanisms and had sought methods of unleashing the productive capacity of the farms. Brannan seemed to show the way. He proposed the maintenance of farm income through direct payments to farmers rather than through crop restriction. In order to encourage and protect the family farm, moreover, he recommended supporting a maximum of about \$26,100 worth of production per farm. To the consumer he promised milk at fifteen cents a quart, to the dairy farmer a sustained high income. To the Democratic party he offered an apparently ingenious device that would unite the interests of farmers and workers.<sup>28</sup>

Liberals generally were enthusiastic over both the principles and the politics of the Brannan proposals. "The new plan lets growers grow and eaters eat, and that is good," commented Samuel Grafton. "If Brannan is right, the political miracle of 1948 will become a habit as farmers, labor and consumers find common political goals," wrote agricultural columnist Angus MacDonald. James Patton called the Brannan Plan "a milestone in the history of American agriculture," and the *Nation* asserted that the average consumer should devote all his spare time to support of the program.<sup>29</sup>

<sup>27</sup> James Loeb to George Jacobson, Feb. 5, 1949; Loeb to James Patton, Feb. 7, 1949; Loeb to Hubert Humphrey, Feb. 17, 1949; Loeb to Joseph P. Lash, Apr. 15, 1949; Loeb, radio speech, Mar. 8, 1949, all in ADA Papers.

<sup>28</sup> Matusow, *Farm Policies and Politics*, 115-19, 194-200.

<sup>29</sup> Grafton in the *Chicago Sun-Times*, Apr. 20, 1949; *St. Louis Post-Dispatch*, Apr. 8, 1949; Angus MacDonald, "The Fair Deal's Farm Program," *New Republic*, May 2, 1949, pp. 11-13; editorial paragraph, *Nation*, Apr. 16, 1949, pp. 429-30; "Patton Praises Principles of Brannan Plan," *National Union Farmer*, Apr. 1949, p. 8.

The plan immediately ran into the opposition of the conservatives who dominated Congress. Republicans feared that the political coalition Brannan was trying to build would entrench the Democrats in power. Large producers, most effectively represented by the powerful Farm Bureau Federation, regarded the plan as discriminatory, and many Democrats with ties to the Farm Bureau refused to support it, among them Senate majority leader Scott Lucas and Clinton Anderson, now the freshman senator from New Mexico. By June it was obvious to most political analysts that the Brannan Plan had no chance of passage in 1949. The administration and most liberals nevertheless remained optimistic. The issue seemed good, the alignment of interests logical and compelling: enough political education and campaigning could revive the scheme and revolutionize American politics.<sup>30</sup>

Both the CIO and the Farmers Union undertook campaigns to spread the message of farmer-labor unity. An article in the *National Union Farmer* typified the effort:

Workers today are in a tough spot, just like farmers. Production has been steadily declining, and that means fewer jobs and lower wages. And that means smaller markets for farm products. This worries everybody but Big Business, but these advocates of scarcity still rule the roost. Monopoly wants less production, less employment, lower wages, fewer family farmers, less collective bargaining, lower farm prices and less competition except for jobs. . . . There is little basic difference between the labor fight against the Taft-Hartley law, and our fight against attempts to tax cooperatives out of existence. . . . Labor's strong objections to 40¢ an hour as a minimum is no different than our equally strong objections to 60% of parity.<sup>31</sup>

Brannan campaigned extensively for his program. "Farm income equals jobs for millions of American workers," he told a labor gathering in a typical effort. "Together, let workers and farmers unite in achieving a full employment, full production economy." The administration sponsored regional farmer-labor conferences around the country. The one attracting the most attention was held in June at Des Moines, Iowa, and featured prominent labor leaders, important Democratic congressmen, and Vice-President Alben Barkley. Other such grass-roots meetings were organized as far east as upstate New York, and the Democratic National Committee prepared a pamphlet on the Brannan Plan for mass distribution. On Labor Day the president devoted two major appearances, one in Pittsburgh and the other in Des Moines, to the Brannan Plan and to farmer-labor unity. "Those who are trying to set these two great groups against each other just

<sup>30</sup> Matusow, *Farm Policies and Politics*, 199–201, 204–19; *Public Papers*, 1949, 292–93.

<sup>31</sup> "Workers Back Brannan Plan," *National Union Farmer*, June 1949, p. 3; "Labor Needs Taft-Hartley Repeal, 75¢ Minimum Wage," *ibid.*, 7; "Hear Labor's View of Brannan Plan. ABC Network, 20th," *ibid.*; "Urge Closer Farm Labor Cooperation," *ibid.*, Aug. 1949, p. 3.



have axes of their own to grind," he warned his Pittsburgh audience. "Price supports must . . . give consumers the benefit of our abundant farm production," he told his Des Moines listeners.<sup>32</sup>

Many liberals and Democratic politicians remained convinced that they had an overwhelming political strategy. "In 1950 and '52, the Brannan Plan will be the great issue in the doubtful states," wrote journalist A. G. Mezerik. "After that, Congress will enact a new farm bill—one which is based on low prices for consumers and a high standard of living for family farmers." In early 1950 the Brannan Plan seemed to be gaining popular support. Liberals inside and outside the administration continued to hope for vindication at the polls in November. They could not, of course, foresee the Korean War and the ways in which it would change the shape of American politics.<sup>33</sup>

Even without the Korean War, however, even without the disruptive impact of McCarthyism, it is doubtful that the Brannan Plan would have worked the miracles expected of it. The liberals inside and outside the administration who had created or worked for it assumed that urban and rural groups could be united simply on grounds of mutual self-interest. They failed to understand that these groups were not deeply concerned with *mutual* self-interest; both sides had practiced with some success methods that had taken care of their own self-interest. The rhetoric about urban-rural interdependence was extremely superficial, talked but not deeply felt. Most farm and labor leaders, even those progressive in their outlook, hardly had a basis for communication. The ADA conference of February 1949 included some of the best-informed figures from the unions and the farms. Yet one of the labor leaders had to ask for an explanation "in simple language" of the concept of parity. One of the farm leaders then admitted that he had no idea what the dues check-off was or how it worked. The farm leaders also frankly commented that their constituents were strongly against such things as a minimum wage applied to farm workers, the extension of social security to cover farm labor and farmers in general, and especially the re-establishment of any sort of price controls. The situation at Des Moines seems to have been much the same. Even some of the Farmers Union officials at the conference were annoyed by the presence of the labor people. "Some farmers wondered if they weren't being sucked in to help the forces of labor fight the Taft-Hartley Act," reported journalist Lauren Soth. Such ideas, of course, were not entirely fanciful. Most of the observers at Des Moines sensed the artificiality of the

<sup>32</sup> Brannan, "The Challenge of Our Era," address delivered to Labor's League for Political Education, in Chicago, Sept. 5, 1949, Clifford Papers; Paul E. Fitzpatrick to Maurice Tobin, May 3, 1950, and Fitzpatrick to Brannan, May 8, 1950, Harry S. Truman Papers, OF 300, Truman Library; Matusow, *Farm Policies and Politics*, 199–200; *Public Papers*, 1949, 460, 467–68.

<sup>33</sup> A. G. Mezerik, "The Brannan Plan," *New Republic*, Nov. 28, 1949, pp. 11–13; Matusow, *Farm Policies and Politics*, 218–21.

whole affair, but they continued to hope that further contacts would consummate the union of city and country.<sup>34</sup>

The farm leaders harbored a provincial suspicion of labor, while the reverse was true in the cities. "While labor has given general support to the Brannan plan, I have had the suggestion made, almost ironically, that labor might be given a guaranteed income if such were to be granted to farmers," remarked Jim Loeb in November 1949. Many liberals felt that, as proposed by the administration, the Brannan Plan was too generous. The Chicago *Sun-Times* and the *Nation* agreed that the principles and machinery of the Brannan system were excellent, but both dissented from Brannan's proposal to support farm income at record heights. "The country as a whole should not undertake to support farm income at a higher level than is fair and just," warned the *Sun-Times*, adding that it would always be easier to raise supports than to lower them. Chester Bowles went a step further when he proposed that the whole matter of agricultural subsidies should be tied to urban employment with no supports at all during periods of full employment. Such ideas were hardly the cement of a new urban-rural coalition.<sup>35</sup>

Many urban liberals found the plan itself difficult to grasp and could not work up much enthusiasm about it. "Most of us do not understand it completely," admitted Jim Loeb a month and a half after its introduction. A group of ADA leaders had a cordial meeting with Brannan in June 1949 and pledged their support. Actually, however, the ADA did little to promote the program. In the spring of 1950 a Philadelphia liberal wrote to the organization asking for information on the issue, but Violet Gunther, the legislative director, replied that the ADA had published nothing other than an endorsement in the platform, nor could she think of any group other than the Farmers Union that might have something available. The *Nation* and the *New Republic* gave only occasional mention to the plan. Most liberals could heartily endorse and even get excited about Brannan's political objectives, but understanding and identifying with the scheme itself was quite a different matter.<sup>36</sup>

For a time in early 1950 declining farm prices seemed to generate a surge of support for the Brannan Plan. At the beginning of June, Albert Loveland, the undersecretary of agriculture, won the Iowa Democratic senatorial primary on a pro-Brannan platform and thereby encouraged the administration to believe that the Midwest was moving in its direction. Just a few weeks later, however, the Korean War began, creating situations and pressures that doomed most of the Fair Deal.<sup>37</sup>

<sup>34</sup> Loeb, radio speech, Mar. 8, 1949, ADA Papers; Milburn P. Akers in the Chicago *Sun-Times*, June 15, 1949; Lauren Soth, "Democrats in Des Moines," *Reporter*, July 19, 1949, pp. 4-7.

<sup>35</sup> Loeb to J. M. Kaplan, Nov. 1, 1949, ADA Papers; Chicago *Sun-Times*, July 12, 1949; "New Farm Plan Needed," *Nation*, June 11, 1949, pp. 649-50; New York *Times*, Apr. 4, 1950.

<sup>36</sup> Loeb to David Williams, May 20, 1949; Loeb to Brannan, June 15, 1949; Violet Gunther to Wilbur Hitchcock, May 10, 1950, all in ADA Papers.

<sup>37</sup> Matusow, *Farm Policies and Politics*, 220-21.

Even if the Brannan Plan had become law, it is far from certain that it would have created the dream farmer-labor-liberal coalition. Most leading agricultural economists, including those of a progressive outlook, were convinced that the proposal would be unworkable and prohibitively expensive. Some liberal economists condemned its failure to give the rural poor at least as much aid as the middle-class family farm.<sup>38</sup> Even assuming that the economists were wrong, there is no guarantee that a smoothly functioning Brannan program could have performed the neat trick of uniting the very different cultures of urban liberalism and rural insurgency; such a feat probably would have required more than mutual economic benefits. The down-to-earth, church-social ethos of the Farmers Union would not automatically homogenize with the sophisticated, intellectual progressivism of the city liberals or the wage-and-hour, union-shop, reformism of labor.

DURING 1949 AND EARLY 1950 the Truman administration managed a record of substantial legislative accomplishment, but it consisted almost entirely of additions to such New Deal programs as the minimum wage, social security, and public power. The Housing Act of 1949, with its provisions for large-scale public housing, appeared to be a breakthrough, but weak administration, local opposition, and inadequate financing subsequently vitiated hopes that it would help the poor. Acting on his executive authority, Truman took an important step by forcing the army to agree to a policy of desegregation. The heart of the Fair Deal, however—repeal of the Taft-Hartley Act, civil rights legislation, aid to education, national medical insurance, and the Brannan Plan—failed in Congress. Given the power of the well-entrenched conservative coalition and a widespread mood of public apathy about big new reforms, Truman could only enlarge upon the record of his predecessor.<sup>39</sup>

Democratic strategists hoped for a mandate in the congressional elections of 1950. In the spring Truman made a successful whistle-stop tour of the West and Midwest, rousing party enthusiasm and apparently demonstrating a solid personal popularity. Loveland's victory provided further encouragement, and in California the aggressive Fair Dealer Helen Gahagan Douglas won the Democratic nomination for the Senate by a thumping margin. Two incumbent Fair Deal supporters—Frank Graham of North Carolina and Claude Pepper of Florida—lost their senatorial primaries, but, as Southerners who had run afoul of the race issue, they did not seem

<sup>38</sup> *Ibid.*, 202–04.

<sup>39</sup> Neustadt, "Congress and the Fair Deal"; Richard O. Davies, *Housing Reform during the Truman Administration* (Columbia, Mo., 1966), 101–42; Richard M. Dalfiume, *Desegregation of the U.S. Armed Forces: Fighting on Two Fronts, 1939–1953* (Columbia, Mo., 1969), ch. 9. On the public apathy toward reform, see Mildred Strunk, ed., "The Quarter's Polls," *Public Opinion Quarterly*, 13 (1949): 154–76, 346–71, 537–61, 709–32.

to be indicators of national trends.<sup>40</sup> Nevertheless, the hope of cutting into the strength of the conservative opposition ran counter to the historical pattern of mid-term elections. The beginning of the Korean War at the end of June destroyed any chances of success.

The most immediate impact of Korea was to refuel an anti-Communist extremism that might otherwise have sputtered out. Senator Joseph R. McCarthy had begun his rise to prominence in February 1950, but he had failed to prove any of his multiple allegations and seemed definitively discredited by the investigations of a special Senate committee headed by Millard Tydings. McCarthy, it is true, was a talented demagogue who should have been taken more seriously by the liberals and the Truman administration in early 1950, but it seems probable that his appeal would have waned more quickly if the cold war with communism had not suddenly become hot. As it was, many of his Senate colleagues rushed to emulate him. In September 1950 Congress passed the McCarran Internal Security Act; only a handful of congressional liberals dared dissent from the overwhelming vote in favor. Truman's subsequent veto was intelligent and courageous, but was issued more for the history books than with any real hope of success. In the subsequent campaign, liberal Democrats, whether they had voted for the McCarran Act or not, found themselves facing charges of softness toward communism.<sup>41</sup>

The war hurt the administration in other ways. It touched off a brief but serious inflation, which caused widespread consumer irritation. By stimulating demand for agricultural products it brought most farm prices up to parity levels and thereby undercut whatever attractiveness the Brannan Plan had developed in rural areas. Finally it removed the Democratic party's most effective spokesman—the president—from active participation in the campaign. Forced to play the role of war leader, Truman allowed himself only one major partisan speech, delivered in St. Louis on the eve of the balloting.

The Fair Deal might have been a winning issue in a nation oriented toward domestic concerns and recovering from an economic recession; it had much less appeal in a country obsessed with Communist aggression and experiencing an inflationary war boom. The reaction against the administration was especially strong in the Midwest. Indiana's Democratic aspirant for the Senate asked Oscar Ewing to stay out of the state. In Iowa, Loveland desperately attempted to reverse his identification with the Brannan Plan. In Missouri the managers of senatorial candidate Thomas

<sup>40</sup> "Exit Senator Pepper," *Nation*, May 13, 1950, pp. 436-37; Carleton Kent, "Harry Goes A-Hunting," *ibid.*, 466-67; "Election Overture," *ibid.*, 514-15; Chicago *Sun-Times*, May 15, 1950; St. Louis *Post-Dispatch*, May 3, June 26, 27, 1950; ADA press release, June 18, 1950, Stephen J. Spingarn Papers, Truman Library.

<sup>41</sup> On the rise of McCarthyism, see Phillips, *Truman Presidency*, 372-93; Alan D. Harper, *The Politics of Loyalty: The White House and the Communist Issue, 1946-1952* (Westport, Conn., 1969), chs. 6-7, apps. 3-4; Robert Griffith, *The Politics of Fear: Joseph R. McCarthy and the Senate* (Lexington, Ky., 1970), chs. 3-5.

C. Hennings, Jr. privately asked White House aides to make Truman's St. Louis speech a foreign policy address that would skip lightly over Fair Deal issues. A few days before the election the columnist Stewart Alsop returned from a Midwestern trip convinced that the region had never been more conservative. Nevertheless, Truman's political advisers, and probably Truman himself, felt that the Fair Deal still had appeal. Given the basic strength of the economy and the victories in Korea that followed the Inchon landing, the White House believed that the Democrats could easily rebut generalized charges of fumbling or softness toward communism. In mid-October the Democratic National Committee and many local leaders were so confident of success that their main concern was simply to get out the vote.<sup>42</sup>

The November results, however, showed a Democratic loss of twenty-eight seats in the House of Representatives and five seats in the Senate. Truman seized every opportunity to remind all who would listen that the numbers were small by traditional mid-term standards. Liberal political analysts, including Kenneth Hechler, a White House staffer, and Gus Tyler of the International Ladies Garment Workers Union, subjected the returns to close scrutiny and all but pronounced a Democratic victory. All the same, most of the Democrats who went under had been staunch Fair Dealers. Republican candidates, including John Marshall Butler in Maryland, Richard M. Nixon in California, Everett McKinley Dirksen in Illinois, and Robert A. Taft in Ohio, scored some of the most spectacular GOP victories by blending right-wing conservatism with McCarthyism. The Midwestern losses were especially disappointing. Hechler argued that the corn-belt vote primarily reflected urban defections and that the Democrats had done comparatively well among farmers. Perhaps so, but for all practical purposes the results put an end to the Brannan strategy of constructing a farmer-labor coalition. Truman was probably more accurate than Hechler when, with characteristic overstatement, he privately expressed his disappointment: "The main trouble with the farmers is that they hate labor so badly that they will not vote for their own interests."<sup>43</sup>

Thereafter, with the Chinese intervention transforming the Korean

<sup>42</sup> Matusow, *Farm Policies and Politics*, 220-21; Stewart Alsop in the *St. Louis Post-Dispatch*, Nov. 4, 1950; George Elsey, memorandum to Charles Murphy, Oct. 30, 1950, Elsey Papers; Kenneth Hechler, memorandums, Aug. 30, Oct. 24, 27, 1950, *ibid.*; David Lloyd, memorandum to Charles Murphy, Sept. 28, 1950, David Lloyd Papers, Truman Library; *Public Papers of the Presidents: Harry S. Truman, 1950* (Washington, 1965), 697-703.

<sup>43</sup> Hechler, memorandum on the 1950 elections, Nov. 15, 1950, Charles Murphy Papers, Truman Library; Matusow, *Farm Policies and Politics*, 220-21; *Public Papers, 1950*, 713; T. R. B., "Washington Wire," *New Republic*, Nov. 20, 1950, pp. 3-4; Harold Ickes, "Fear Rides Herd," *ibid.*, 17; Gus Tyler, "The Mid-Term Paradox," *ibid.*, Nov. 27, 1950, pp. 14-15; Thomas L. Stokes in the *Washington Evening Star*, Nov. 13, 1950; W. McNeil Lowry, "Frustration in the Corn Belt," *Progressive*, Dec. 1950, pp. 14-16; James Patton, "'The People Have Spoken,'" *National Union Farmer*, Nov. 1950, pp. 1, 2; Elmer Davis, news commentary, Nov. 8, 1950, Elmer Davis Papers, Library of Congress; Truman to Aubrey Williams, Nov. 18, 1950, Aubrey Williams Papers, Franklin D. Roosevelt Library, Hyde Park, N.Y.

War into a more serious conflict and with the dismissal of General Douglas MacArthur in April 1951, Truman faced a tough attack from a Republican opposition determined to capitalize upon the frustrations of Korea. Finding it necessary to place party unity above all else, he quietly shelved most of his domestic legislative program and sought to bring the conservative wing of his party behind his military and defense policies. He secretly asked Richard B. Russell of Georgia, the kingpin of the Southern conservatives, to assume the Democratic leadership in the Senate. Russell, content with the substance of power, declined and gave his nod to Ernest W. McFarland of Arizona, an amiable tool of the Southern bloc; Truman made no effort to prevent McFarland's selection as Senate majority leader. The president's State of the Union message was devoted almost entirely to foreign policy and defense mobilization and mentioned social welfare programs only as an afterthought. Subsequently Truman told a press conference that while he supported the Fair Deal as much as ever, "first things come first, and our defense programs must have top priority."<sup>44</sup>

Truman's success in achieving a minimum degree of party unity became apparent in the weeks of investigation and accusation that followed General MacArthur's return to America. Russell, playing the role of parliamentarian-statesman to the hilt and cashing in on his great prestige with senators of both parties, chaired the Senate committee that looked into the MacArthur incident, and he saw to it that the administration was able to deliver a thorough rebuttal to the general. The Northern liberal, Brien McMahon of Connecticut, relentlessly grilled hostile witnesses. The Western representative of oil and gas interests, Robert S. Kerr of Oklahoma, lashed out at MacArthur himself with a vehemence and effectiveness that no other Democrat could match. The tandem efforts of Russell, McMahon, and Kerr demonstrated the new party solidarity, but in terms of the Fair Deal the price was high.

In July 1951 the Federal Power Commission renounced the authority to regulate "independent" (non-pipeline-owning) natural gas producers. The ruling amounted to an administrative enactment of a bill, sponsored by Kerr, which Truman had vetoed a year earlier; Truman's close friend and most recent appointee to the Federal Power Commission, Mon Wallgren, cast the deciding vote. Although he talked like a militant liberal in a private conversation with ADA leaders, the president stalled throughout 1951 on repeated demands for the establishment of a Korean War Fair Employment Practices Committee. In December the administration established an ineffective Committee on Government Contract Compliance. Other domestic programs were soft-pedaled to near-invisibility.<sup>45</sup>

Yet even the Korean War was not entirely inimical to reform. Its exi-

<sup>44</sup> Rowland Evans and Robert Novak, *Lyndon B. Johnson: The Exercise of Power* (New York, 1966), 41-43; *Public Papers of the Presidents: Harry S. Truman, 1951* (Washington, 1965), 6-13, 18, 22; Poen, "The Truman Administration and National Health Insurance," 214-19; memorandums in "Taft-Hartley-1952" file, David Stowe Papers, Truman Library.

<sup>45</sup> Douglass Cater, "A Parliamentarian, A Hatchet Man, an Inquisitor," *Reporter*, June 12,



gencies forced the army to transform its policy of integration into practice.<sup>46</sup> Korea also provided a test for one of the basic underpinnings of the Fair Deal—Leon Keyserling's philosophy of economic expansion. Truman did not in the end fully embrace Keyserling's policies, but in the main he followed the guidance of his chief economic adviser. The Korean War years demonstrated the extent to which Keyserling's economics diverged from conventional New Deal–World War II Keynesianism and revealed both the strengths and weaknesses of his approach.

From the outbreak of the fighting, most liberals favored either immediate strong economic controls akin to those that had held down inflation in World War II or at least the establishment of stand-by machinery that could impose them rapidly. Truman disliked such measures on the basis of both principle and politics. He and his diplomatic advisers also wanted to signal the Soviet Union that the United States regarded the North Korean attack as a limited challenge meriting a limited response. Keyserling's expansionary economics provided an attractive alternative to the liberal clamor for controls. Convinced that extensive controls would put the economy in a strait jacket and retard the expansion necessary to meet both consumer and defense needs and assuming a North Korean defeat in a few months, the administration decided to accept a short-term, war-scared inflation (probably unavoidable in any case) and concentrate on economic growth, which would be underwritten in large measure by tax incentives for business. An expanding economy would be the best long-term answer to inflation: growth policies could fit a small war into the economy, avoid the social and political strains accompanying wartime controls, and reduce inflationary pressures to a level at which fiscal and monetary policies could contain them. Liberals outside the administration watched with alarm as prices went up, but Truman and Keyserling continued to gamble on a quick end to the war and the development of an economy capable of producing both guns and butter.<sup>47</sup>

Their plan might have worked fairly well had the United States not

1951, pp. 31–32; Willard Shelton, "Presidential Appointments," *Nation*, Feb. 16, 1952, p. 149; "Costly Alliance," *Progressive*, Dec. 1951, p. 3; St. Louis *Post-Dispatch*, July 20, 22, 1951; Francis Biddle to Truman, May 22, 1951, ADA Papers; Biddle to Emil Rieve, June 14, 1951, *ibid.*; John Tucker to Biddle, Aug. 16, 1951, *ibid.*; Violet Gunther, memorandum to Reginald Zalles [early Nov. 1951], *ibid.*; Herbert Garfinkle, *When Negroes March* (New York, 1969), 176–77. Although there can be no doubt that the administration stalled on the civil rights order for political reasons, it is also true that the twilight nature of the Korean War placed formidable legal difficulties in the way of a strong Fair Employment Practices Committee. See Fred Lawton, memorandum for the record, Feb. 1, 1951, Fred Lawton Papers, Truman Library; Charles Murphy, memorandum to Truman, Dec. 1, 1951, Truman Papers, OF 40; "FEPC for Defense," *New Republic*, Dec. 17, 1951, p. 6.

<sup>46</sup> Dalfiume, *Desegregation of the U.S. Armed Forces*, ch. 10.

<sup>47</sup> For the administration position on controls, see Edward S. Flash, Jr., *Economic Advice and Presidential Leadership: The Council of Economic Advisers* (New York, 1965), 43–44; *Public Papers, 1950*, 561–64, 568–69, 589–90; Richard Neustadt, memorandum to Kenneth Hechler, Apr. 28, 1952, Elsey Papers; and author's interview with Keyserling. For liberal criticism, see, e.g., Chester Bowles to Burnet R. Maybank in the *Congressional Record*, 81st Cong., 2d sess., Aug. 4, 1950, pp. 11825–26; ADA mobilization program, *ibid.*, 11826–27; Hubert Humphrey in *ibid.*, app., pp. A5647–49; St. Louis *Post-Dispatch*, July 28, 1950; "Washington Fiddles," *Nation*, Aug. 26, 1950, pp. 178–79; "Gallop-ing Inflation," *New Republic*, Oct. 2, 1950, pp. 5–6.

overreached itself militarily in Korea. The Chinese intervention of November 1950 wrecked hopes of a quick recovery, set off another round of scare buying, and intensified war demands upon the economy. The administration quickly threw up a price-wage control structure, but by the end of February 1951, eight months after the beginning of the Korean conflict, the consumer price index had risen eight per cent (an annual rate of twelve per cent). Keyserling agreed that the new situation necessitated controls, but he accepted them with reluctance and sought to keep them as simple as possible, even at the risk of benefiting profiteers. "We'll never be able to out-control the Russians," he told a Senate committee, "but we can out-produce them." Speaking to an ADA economic conference, he asserted that many liberals, in their opposition to tax breaks for large business and in their demands for stronger controls, were confusing the Korean War with World War II and "engaging merely in hackneyed slogans out of the past."<sup>48</sup>

Most liberals disagreed with Keyserling's emphases. As production was his first imperative, an end to the wage-price spiral was theirs. "Unless we are willing seriously to endanger the basis of existence of the American middle class, we must stop prices from rising," wrote Hans Landsberg in the *Reporter*. The liberals assumed that economic expansion was possible within a framework of rigid, tightly administered controls. Chester Bowles observed that the controlled economy of World War II had turned out a twofold increase in industrial production. John Kenneth Galbraith rejected the idea that Keyserling's expansionary policies could outrun the inflationary pressures they themselves created. The bulk of liberals regarded the administration approach as dangerous, the product of political expediency rather than sound economic analysis.<sup>49</sup>

Neither Keyserling nor the more conventional liberals won a complete victory. Truman, who understood all too well the political dangers of a prolonged inflation, made substantial concessions to the controllers, led by Michael V. DiSalle, head of the Office of Price Stabilization. In the interest of fairness Truman approved a more complex system of price controls than Keyserling thought desirable, giving DiSalle considerable leeway to roll back some prices while approving advances in other areas. By March 1951 inflation was under control; during the final ten months of the year the cost-of-living index increased by less than two and one-half per cent. The waves of scare buying that followed the North Korean attack and

<sup>48</sup> Flash, *Economic Advice and Presidential Leadership*, 69-76; *New York Times*, Nov. 30, Dec. 11, 12, 1950, May 10, 1951; Keyserling, speech to ADA conference, Washington, D.C., May 18, 1951, ADA Papers; Harold Enarson, memorandum to David Stowe, Apr. 11, 1951, Harold Enarson Papers, Truman Library.

<sup>49</sup> Landsberg, "Who Pays for Mobilization?" *Reporter*, Mar. 20, 1951, pp. 6-8; Galbraith, "The Taxonomy of Inflation Control," *ibid.*, July 10, 1951, pp. 9-11; Bowles, public letter to Paul H. Douglas, May 31, 1951, copy attached to Bowles to Charles Murphy, June 6, 1951, Murphy Files, Truman Papers.

the Chinese intervention had subsided. Higher taxes and restraints on credit were beginning to affect consumer buying. The Federal Reserve System, despite opposition from the administration, initiated a stringent monetary policy. Tax breaks for businesses expanding plant facilities presaged increased productive capacity. All these factors, along with the government stabilization program, discouraged an inflationary psychology.<sup>50</sup>

At the time, however, it appeared to most economic observers that the lull was only temporary. Many of the administration's liberal critics refused even to admit the existence of a lull and called for tougher controls as if prices were still skyrocketing. More moderate analysts feared that the impact of large government defense orders would set off another inflationary spiral in the fall. Influenced by such expectations, Truman ostentatiously mounted an anti-inflation crusade, demanding that Congress not only extend his control authority, due to expire on June 30, but actually strengthen it. In fact the Defense Production Act of 1951 weakened the president's powers considerably. Truman signed it reluctantly, comparing it to "a bulldozer, crashing aimlessly through existing pricing formulas, leaving havoc in its wake." A subsequent tax bill failed to meet administration revenue requests and increased the danger of serious inflation.<sup>51</sup>

Yet price stability persisted through 1952, in large measure because defense production, hampered by multiple shortages and bottlenecks, lagged far behind its timetable. In late 1951 these problems and the fear of renewed inflation led Truman to decide in favor of a "stretch-out" of defense production schedules; in doing so he overrode Keyserling's urgings for an all-out effort to break the bottlenecks and concentrate relentlessly upon expansion. Given the serious problems in defense industry, the stretch-out decision may have seemed necessary to Truman, but it also carried the dividend of economic stability.

The president had steered a course between the orthodox liberal obsession with inflation and Keyserling's easy disregard of its perils; perhaps as a result the economy failed to expand at the rate Keyserling had hoped. On balance, however, Truman's approach to the political economy of the Korean War was closer to Keyserling's, and the conflict produced a dramatic economic growth. Before the war the peak gross national product had been \$285 billion in 1948; by the end of 1952 the GNP (measured in constant dollar values) had reached a rate of \$350 billion. The production index of durable manufactured goods had averaged 237 in 1950; by the last quarter of 1952 it had reached 313. The expansion, even if less than

<sup>50</sup> Arthur Viner, "What Happened to Inflation?" *Reporter*, Apr. 15, 1952, pp. 32-34; *Statistical Abstract of the United States, 1953* (Washington, 1953), 307.

<sup>51</sup> Examples of continued liberal concern about inflation are Thomas L. Stokes in the *Washington Evening Star*, Apr. 4, 1951; ADA press release, Apr. 16, 1951, ADA Papers; "Are We Licking Inflation?" *New Republic*, Apr. 30, 1951, p. 6; and *St. Louis Post-Dispatch*, June 12, 1951. For Truman's course, see *Public Papers, 1951*, 244-53, 320-21, 333-38, 364, 369, 435-37, 478-83, 590.

Keyserling had wanted, was breathtaking. Moreover, aside from the probably unavoidable inflation that accompanied the early months of the war, this remarkable growth had occurred in a climate of economic stability. Using a somewhat more orthodox approach than Keyserling preferred, the administration had achieved one of the central goals of the Fair Deal.<sup>52</sup>

In its effort to carry on with the reforming impulse of the New Deal the Truman administration faced nearly insuperable obstacles. A loosely knit but nonetheless effective conservative coalition had controlled Congress since 1939, successfully defying Franklin Roosevelt long before it had to deal with Truman. Postwar prosperity muted economic liberalism and encouraged a mood of apathy toward new reform breakthroughs, although Truman's victory in 1948 indicated that most of the elements of the old Roosevelt coalition were determined to preserve the gains of the New Deal.<sup>53</sup> The cold war probably made it more difficult to focus public attention upon reform and dealt severe blows to civil liberties. It did, however, give impetus to the movement for Negro equality.

The Fair Deal attempted to adapt liberalism to the new conditions. Under the intellectual leadership of Leon Keyserling it formulated policies that sought to transcend the conflicts of the New Deal era by encouraging an economic growth that could provide abundance for all Americans. With Charles Brannan pointing the way, the Truman administration tried to translate abundance into a political coalition that could provide the votes for its social welfare policies. The political strategy, ambitious but unrealistic, collapsed under the weight of the Korean War. Keyserling's economics, on the other hand, received a lift from Korea; in a period of adversity the Fair Deal was able to achieve at least one of its objectives.

<sup>52</sup> *Statistical Abstract, 1953*, 786; Flash, *Economic Advice and Presidential Leadership*, 85-99; *Public Papers of the Presidents: Harry S. Truman, 1952-53* (Washington, 1966), 1179.

<sup>53</sup> Samuel Lubell, *The Future of American Politics* (3d ed. rev.; New York, 1965), remains the most influential analysis of the deadlock of the Roosevelt coalition.

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# Marx and the Agrarian Question

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OSCAR J. HAMMEN

THE FIRST SUCCESSFUL REVOLUTION avowedly inspired and guided by the ideology and historical interpretation of Karl Marx and Friedrich Engels took place in Russia in 1917. Since this event occurred in a mainly agricultural state with a relatively small industrial establishment, it is often listed as one of the ironies of history. This has led some scholars to the conclusion that Lenin, the director of the Bolshevik forces, gave an un-Marxian twist to Marx, deviating from the theories of the master in the process.

The theories of Marx and Engels certainly stressed the primary revolutionary role of an industrial proletariat in the more advanced capitalistic states. Marx and Engels in their critiques also depended heavily on the historical experience and the contemporary condition of England in demonstrating the inherent contradictions and class conflicts that resulted from changing modes of production.<sup>1</sup> With the expansion of industry and the application of capital, associated as they were with the presence of an ever-growing and more numerous proletariat, the capitalist system produced its own hangmen. The above line of thought also reflected an application of Marxian dialectics, with an emphasis on the clash of opposites, class conflict, and the advance of civilization proceeding from this logic.

It must be noted, though, that Marx and Engels were so certain that the course of history pointed toward the ultimate triumph of communism that they were ready to support any revolutionary movement that promised to eliminate a variety of roadblocks (political, economic, and social institutions, and even "reactionary nations") that needed to be removed to clear the way for the last climactic confrontation between proletariat and bourgeoisie. A pattern that such upheavals were likely to follow was always sought by Marx in the great French Revolution—a revolution that had

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<sup>1</sup> Beginning with Engels's "Umriss zu einer Kritik der Nationalökonomie," in the *Deutsch-Französische Jahrbücher* (Paris, 1844), followed by *Die Lage der Arbeitenden Klasse in England* (Leipzig, 1845), and culminating in Marx's *Kapital* (Hamburg, 1867).

followed an "ascending line," as the "more progressive parties" pushed their predecessors aside and sent them to the guillotine.<sup>2</sup> A personal factor also entered the picture, because Marx and Engels were anything but mere theorists content to watch history follow its own fumbling but destined path in the ripeness of time. They expected to witness the predicted revolutions and to participate directly in them.<sup>3</sup> This helps to account for their unmatched display of "tactical shuffling," as Joseph A. Schumpeter once called it,<sup>4</sup> their readiness to identify themselves with the grievances of diverse groups—worker, agrarian, national, political, economic, and even religious—that promised to advance the "movement" (*Bewegung*, a favorite expression of Marx and Engels).

Scholars who are interested primarily in the theoretical side of Marx and Engels are apt to frown on any undue, or perhaps merely proportionate, emphasis on Marx as an active promoter of revolutions, with all the tactical adjustments to diverse national settings and local circumstances that this entailed. Such a stress on tactics appears to be a derogation from the role that theory occupied in the thoughts of Marx and Engels. Actually it underlines the importance of the theoretical assumptions. Marx and Engels could recommend and practice all manner of deviations to promote the destruction of an existing system precisely because they were completely confident that their theoretical conclusions were infallible, that they moved with the stream of history. The communists who comprehended the "movement" remained in a position to guide events in the destined historical direction. This confidence persisted even though Marx ultimately shed some of the "comfortable delusions and the almost childish enthusiasm" with which Marx and Engels had greeted the 1848 revolutions. Marx, moreover, had come to recognize the "role that stupidity plays in revolutions and how the rascals [*Lumpen*] exploit it."<sup>5</sup>

<sup>2</sup> Marx, "Der achtzehnte Brumaire des Louis Bonaparte," *Karl Marx/Friedrich Engels: Werke* (hereafter *Werke*) (Berlin, 1956–68), 8: 135.

<sup>3</sup> Their commitment to revolutionary involvement in the "movement" is illustrated by the expectations expressed in section 4 of the *Communist Manifesto* and in their participation in the revolutions of 1848–49, notably in Germany. In the last "red" number of the *NRZ* (*Neue Rheinische Zeitung. Organ der Demokratie*), May 19, 1849, they promised their followers that they would return when the action started again. Their hopes for an early return to the continent following another eruption of the "revolutionary volcano" persisted into 1850 and are well documented. The Marx-Engels correspondence (*Briefwechsel*) in the years that followed demonstrates the same expectations. See the following representative letters: Engels to Marx, Nov. 27, 1851; Marx to Engels, Mar. 5, 1856; Marx to Engels, Sept. 26, 1856; Engels to Marx, Feb. 11, 1858; Marx to Engels, Feb. 14, 1858; Engels to Marx, Apr. 11, 1859; Engels to Marx, Apr. 10, 1866; Engels to Marx, Nov. 29, 1868. The *Briefwechsel*, aside from special editions, appears in the four volumes in part 3 of *MEGA*, *Karl Marx/Friedrich Engels. Historisch-Kritische Gesamtausgabe. Werke/Schriften/Briefe* (Frankfurt, Berlin, Moscow, 1927–35). References in this article will be to the *MEGA* publication as follows: *Briefwechsel*, 1, 2, 3, or 4. The *Briefwechsel* also appears in volumes 27–35 of the *Werke*. Engels's testimonial at the graveside of Marx in 1883 stresses the fact that the "scientist and theorist in Marx represented only half of the man." See Engels, "Das Begräbnis von Karl Marx," in *Werke*, 19: 335–37.

<sup>4</sup> Joseph A. Schumpeter, *Capitalism, Socialism and Democracy* (3d ed.; New York, 1950), 319.

<sup>5</sup> Marx to Engels, Feb. 13, 1863, in *Briefwechsel*, 3: 126–27.



Scholars by and large have neglected the systematic study of the direct participation of Marx and Engels in an actual revolution<sup>6</sup> together with their capacity for tactical adjustments—a capacity that was demonstrated repeatedly and that would give the two men a role in a great variety of revolutionary movements without ever causing them to lose sight of their ultimate goals. Yet any appraisal of Marx and Engels and what they represented would appear to be unrealistic and lopsided in the absence of the above considerations. Marxism represents an amalgam of theory and practice.

Marx and Engels, in any event, did not consistently abide by their own apparent theoretical conclusions that pointed to the proletariat in the most advanced industrial lands for the initiative in the communist movement. They occasionally indicated that the virgin soil of a belatedly emerging proletariat constituted a richer seed bed. In the *Communist Manifesto* of 1848 they predicted the early success of a bourgeois revolution in Germany as the “immediate prelude” to a succeeding proletarian revolution. They justified this prediction by asserting that the German upheaval would occur under “generally more advanced conditions of European civilization and a far more developed proletariat” than had existed during comparable English and French revolutions in the seventeenth and eighteenth centuries.<sup>7</sup> With the added maturity of twenty years Marx in 1868 again reflected on the advantages of a late start. Beyond that he then gave the Germans credit for a special national trait: They “had on their shoulders heads that could generalize.”<sup>8</sup> The Germans were the “most theoretical people in Europe and had preserved a theoretical frame of mind [*Sinn*],” Engels wrote concurringly. The lack of similar “frame of mind” among the English and French accounted for their relative insensitivity to Marxian views.<sup>9</sup>

<sup>6</sup> This neglect is most apparent in connection with the role of Marx and Engels in the revolutions of 1848–49, the only revolutions in which they were involved directly and to which they continued to refer as a cardinal event in their lives. Among the more comprehensive but also belated studies, the centenary account by Auguste Cornu, *Karl Marx et la Révolution de 1848* (Paris, 1948), is relatively elemental and thin (merely seventy-four pages). Gerhard Becker's *Karl Marx und Friedrich Engels in Köln 1848–1849* (Berlin, 1963) is scholarly, reasonably comprehensive and commendable, though colored by an obvious communist bias. The only relatively thorough study in the English language is by the author: Oscar J. Hammen, *The Red '48ers: Karl Marx and Friedrich Engels* (New York, 1969).

<sup>7</sup> “Manifest der Kommunistischen Partei,” pt. 4, in *Werke*, 4: 493.

<sup>8</sup> Marx to Engels, Sept. 26, 1868, in *Briefwechsel*, 4: 102.

<sup>9</sup> Engels, “Vorbemerkung zu ‘Der deutsche Bauernkrieg’ (Ausgabe 1870 und 1875),” in *Werke*, 7: 541. Engels spoke later of “two essential advantages” the German workers enjoyed in contrast to other workers. First, as members of the “most theoretical people in Europe,” they had preserved a “theoretical frame of mind.” Without it “scientific socialism” would never have become so much a part of their “flesh and blood.” The absence of this “inestimable advantage” was a major cause of the “indifference toward theory” that accounted for the fact that the English workers “moved so slowly from the spot” as it also accounted for the confusion and mischief that resulted from the French and Belgian dedication to “Proudhonism” and the “caricatured form” of the latter that had been transmitted to the Spaniards and Italians by Bakunin. See Engels to Bebel, Dec. 11, 1884, in *Karl Marx and Friedrich Engels, Correspondence*, (hereafter *Correspondence*) (New York, 1934), 431–32. August Bebel was a noted German Social Democratic leader.

Marx and Engels also declared that the misery of the workingman in the industrially backward countries and regions was more pronounced than in the advanced states, thereby suggesting a perhaps added responsiveness to radical promptings. Engels informed a German audience that the Germans experienced in a more acute form the same economic crises that struck the English.<sup>10</sup> Workers in backward areas also were drawn into the "movement" by the real industrial proletariat elsewhere, Marx asserted. The industrially advanced states produced the same internal "contradictions" (conflict of classes) in the less industrialized countries because of the competition resulting from the expansion of international trade—"the latent proletariat in Germany," for example, was "created by the competition of English industry."<sup>11</sup> By 1847, therefore, Germany had already experienced to a degree the "modern conflict between the bourgeoisie and the working class, and the battle resulting from it, because of her industrial development and German dependence on the world market."<sup>12</sup> The great economic crises, or depressions, likewise affected the backward countries with an equal if not greater force. "It is known," Marx stated in 1850, "that no European land is struck so directly, with the same scope and intensity, as Germany by the effects of an English crisis."<sup>13</sup> It must be noted here that Marx and Engels counted much on the revolutionary impulse resulting from an economic crisis, especially after the experiences of 1848–49. Such crises revealed the contradictions in a capitalist society in a most glaring manner, while simultaneously sharpening the misery of the proletariat and increasing its alleged readiness to revolt against the prevailing system.

It should be evident from the preceding that Marx and Engels were not unduly hemmed in by the apparent implications of their theories that seemed to suggest that the communist, the proletarian, victory was destined to be won first in the most advanced industrialized states. In actual practice Marx and Engels saw certain possibilities in the rela-

<sup>10</sup> Engels, "Zwei Reden in Elberfeld II," in *Werke*, 2: 551–52.

<sup>11</sup> Marx and Engels, "Die deutsche Ideologie," in *ibid.*, 3: 61, 73.

<sup>12</sup> Marx, "Die moralisierende Kritik und die kritisierende Moral," *Deutsche-Brüsseler-Zeitung (DBZ)*, Nov. 18, 1847, in *ibid.*, 4: 351.

<sup>13</sup> Marx and Engels, *Neue Rheinische Zeitung. Politisch-ökonomische Revue* (hereafter *Revue*) (Hamburg, 1850), in *Werke*, 7: 293–94. After many additional years of practical observation Engels noted some added Marxian gains that resulted from a belated industrial development. A great advantage of the Germans could be ascribed to the fact that the "industrial revolution is only just in full swing, while in France and England, so far as the main point is concerned, it is closed." In the latter countries population shifts took place only very slowly; the great mass of the people grew up "in the conditions in which they have later to live, are accustomed to them; even the fluctuations and crises have become something they take practically for granted." Moreover, they were burdened with the memory of past "unsuccessful attempts." Germany, in contrast, lagged behind, and consequently the social upheaval was all the more fundamental. Germany had a "perfectly fresh and intact proletariat, undemoralized by defeats and finally—thanks to Marx—with an insight into the causes of economic and political development such as none of our predecessors possessed." Letter to Bebel, Dec. 11, 1884, in *Correspondence*, 431–32.

tively backward states. The Germans, as noted above, showed a greater philosophical capacity, the ability to “generalize,” plus a more retarded development that added to their miseries. The French, on the other hand, possessed a marked revolutionary tradition.

The less industrialized states lacked the vast and numerous industrial proletariat that could be organized to serve as the shock troops of revolution all the more readily because of their concentration in the large cities, often the capital cities. But this deficiency could be offset through tactical alliances with other potentially revolutionary elements. In the large agricultural states (most of the European states) it might be possible to turn to the agrarian question covering a host of grievances and expectations experienced by the rural people, in the hope that agrarian discontent, guided by the ideologically more sophisticated proletariat, could be mobilized for revolution. Marx and Engels always believed that the proletariat, as the class that would ultimately triumph, would play the leading role in such combinations—if not at once, then later.

In writing on Marx and the agrarian question here the greater stress will be on the earlier period extending into the 1850s. This was the time when tactical considerations were more pronounced because of the presence of actual revolutions, which were followed by reflections on the miscarriage of those revolutions and the anticipation of early revolutions to follow. When immediate revolutionary prospects were dim Marx concentrated more strongly on historical and theoretical investigations, so as to make them available as guidelines for the “deluge” to come. In any event, it is impossible within the scope of an article to provide the same coverage for the later and longer period. Enough will be added, however, to suggest that Marx and Engels adhered to the same tactical pattern in the times that followed—despite repeated disappointments resulting from the peasant’s tendency to act in a conservative and even “reactionary” manner.

From a theoretical point of view Marx and Engels always felt that the peasant, farm tenant, and agricultural worker were subjected to the same nightmare of exploitation by the bourgeois, capitalist society as were the proletariat, even if in a less apparent, direct, and massive manner. Engels, who devoted a chapter in his first major work, *The Condition of the Working Class in England*, to “The Agricultural Proletariat,” explained that as the machine had given birth to an industrial proletariat, so it had also produced an agricultural proletariat, which was employed on big land units owned or rented by large-scale farmers who utilized the latest machines. The lot of day laborers on the land appeared especially miserable; they would become “Chartists and socialists, that is, conscious proletarians.” As for the tenant farmer who cultivated a few acres of leased land (in Wales and Ireland), he faced the same ruin that hounded the petty bourgeoisie in the cities. The resulting agrarian dis-

content manifested itself initially in the form of violence and incendiarism.<sup>14</sup> "The abolition of capital . . . is the property question in the sense of the English agricultural day laborer just as it is of the factory worker," Marx asserted.<sup>15</sup> There was little need of speaking of the once proud English "yeomanry"—as a nearly extinct species.

But the small peasant, wherever he existed, was likewise the victim of an inescapable capitalist exploitation and of the forces of competition. France, where two-thirds of the population belonged to the ranks of the peasantry, furnished the classical model to illustrate the situation. But peasants in the Prussian Rhine Province and certain other areas in Germany, as well as in other countries, faced the same problem. Speaking of France, Marx in 1848 insisted that the "rule of the feudal lords has been supplanted by that of the capitalist; the feudal obligations of the peasant have been converted into the burden of the bourgeois mortgage." Agriculture could not flourish under existing civilized conditions in which the peasant was sinking into an increased poverty.<sup>16</sup>

The continued "parceling" or subdivision of peasant holdings, accompanied by the rising price of land, brought a proportionate increase in debt in the form of the mortgage. At the same time the productivity of the land declined; it was impossible to use machinery and other modern devices to improve the yield of such small farms. Each generation started with the burden of a greater debt. One mortgage followed the other until the peasants had to turn to the usurer with his exorbitant interest rates, all in the name of private property. "Only the fall of the capitalist can help the peasant," Marx claimed. "Only an anti-capitalist, a proletarian government can end his economic misery, his social degradation."<sup>17</sup> Marx also expected to cover some aspects of the agrarian question in a series of articles on "Wage Labor and Capital" that began appearing in the *Neue Rheinische Zeitung (NRZ)* in 1849. There he promised to examine the subject under three subdivisions, item two being the "inescapable fall of the middle bourgeois classes and the peasant class under the present system."<sup>18</sup> But Marx typically never got beyond the first item in the projected series. Since the small peasant proprietor, theoretically considered, could never prosper or even survive indefinitely under the capitalist system, it was evident that he might be persuaded to support a proletarian revolution, once the communists succeeded in clarifying his thoughts on the subject and in making him conscious of his

<sup>14</sup> See Engels, "Das Ackerbauproletariat," *Die Lage der arbeitenden Klasse in England. Nach eigener Anschauung und authentischen Quellen*, chap. 4 (Leipzig, 1845), in *Werke*, 2: 473–85.

<sup>15</sup> Marx, "Moralisierende Kritik," in *ibid.*, 4: 341.

<sup>16</sup> Marx, "Thiers' Rede über eine allgemeine Hypothekenbank mit Zwangkurs," *NRZ*, Oct. 14, 1848, in *ibid.*, 5: 424–25.

<sup>17</sup> Marx, "Die Klassenkämpfe in Frankreich 1848 bis 1850," *Revue*, in *Werke*, 7: 83–84.

<sup>18</sup> Marx, "Lohnarbeit und Kapital," *NRZ*, Apr. 5, 1849, in *ibid.*, 6: 398.

hopeless position. But this appeared a hopeless task, because the peasant clung to his parcel of land and hoped for better times and perhaps more land. The more practical approach to the creation of an alliance for revolutionary purposes demanded that the communists see the world through the eyes of the peasants. The communists had to win the peasants by tactically identifying themselves with the various agrarian demands, hopes, and grievances. Marx and Engels early recognized the value of going to the land.

The first illustration of this fact spelled out in a formal manner appeared in the "Circular against Kriege," drafted in 1846 by a forerunner of the Communist League, the Communist Correspondence Committee, with its headquarters in Brussels. Hermann Kriege, whom Engels originally sent to Marx as a promising recruit,<sup>19</sup> displayed inadmissible deviations from the correct views after he settled in New York and published a newspaper. The "Circular," after citing many other errors in Kriege's views, attacked his "economics" for supporting a program advocated by "Young America" and favoring the distribution of 160 acres of land and no more to all farmers.

Had Kriege supported the free-soil movement under specific conditions, had he presented it as a movement . . . that necessarily had to lead to communism, had he shown how the communist tendencies in America originally had to present themselves in this agrarian manner that seemingly contradicted communism as such, there would be no cause for complaint,

the "Circular" explained.<sup>20</sup> The talent for tactical elasticity displayed here—a reflection of confidence in the dialectical process—remained a pronounced characteristic of Marx and Engels. They advised another German follower thus: "In a party one must support everything that helps, without having any boring scruples there."<sup>21</sup> In 1848 Marx declared that in the struggle against the existing government (in this case Prussia) they "allied themselves even with [their] enemies." They had accepted the miserable make-up of the opposition for what it was and had allowed their own views to recede into the background in an election that had just been held.<sup>22</sup> In a slightly lighter vein Marx in 1852 wrote that it was permissible to ally oneself with the devil in order to achieve a specific goal in politics—but one had to be sure that one was getting the best of the devil and not the reverse.<sup>23</sup> That Marx and Engels continued to show the same readiness to make tactical adjust-

<sup>19</sup> Engels to Marx, Feb. 22–Mar. 7, 1845, in *Briefwechsel*, 1: 13–14. Engels described Kriege as a "stupendous agitator."

<sup>20</sup> Marx, Engels, *et al.*, "Zirkular gegen Kriege," May 11, 1846, in *Werke*, 4: 8–10.

<sup>21</sup> Marx, Engels, *et al.*, "Brief des Brüsseler kommunistischen Korrespondenz-Komitees an G. A. Köttgen," June 15, 1846, in *ibid.*, 22.

<sup>22</sup> Marx, "Stein," *NRZ*, Feb. 18, 1849, in *ibid.*, 6: 298.

<sup>23</sup> Marx, "Kossuth, Mazzini und Louis-Napoleon," in *ibid.*, 8: 392. The original appeared in the form of a letter to the editor of the New York *Daily Tribune*, Dec. 1, 1852.

ments to existing circumstances is borne out in their correspondence (*Briefwechsel*).

In preparation for the expected revolutions that came in 1848 Marx and Engels posed as democrats. The "democratic party" of which they spoke included the broad masses: workers, the petty bourgeoisie, and the most numerous class, the peasantry. Marx, in addressing the Germans, presented himself also as the champion of the people (*Volk*), though he once suggested that the word "proletariat" be used in place of that "broad, indefinite expression."<sup>24</sup> Engels expressed it more bluntly: "*Democracy, that today means communism*. Any other democracy can exist only in the heads of theoretical visionaries." The party of the "Terror" of 1793 in France had found its support in the "rebellious proletariat."<sup>25</sup> "The industrial proletariat of the cities has become the crown of all modern democracy," Engels stated elsewhere; "the petty bourgeoisie and even more the peasants depend completely on its initiative." The small peasants were the least capable of taking the "revolutionary initiative."<sup>26</sup> Marx and Engels apparently felt sure that the communists would fill the gap.

The "Principles of Communism," Engels's draft of a communist "catechism" or "confession of faith" whose basic points thereafter reappeared in the *Manifesto* of 1848, touched up or toned down and supplemented by Marx, stated that the revolution would result in a "*democratic constitution*." That meant the "direct or indirect political rule of the proletariat." The rule would be indirect in France and Germany, where the proletariat was still the minority and where the majority was made up of small peasants and petty bourgeoisie. But the last two, who were "just in the process of a transition into the proletariat" and whose "political interests depended more and more on the proletariat," soon would have to yield to the demands of the workers. "Democracy would be wholly useless for the proletariat unless it immediately serves as a means for the implementation of measures attacking private property and securing the position of the proletariat."<sup>27</sup> The *Manifesto* later was less explicit.

In the last part of the *Communist Manifesto* Marx outlined the tactics of the party in the coming revolutions. The tactics varied greatly because they were attuned to the actual conditions and possibilities, as Marx and Engels saw them, in each land. In Poland they supported the party that insisted on an "agrarian revolution" as a prerequisite for national liberation. In the United States they endorsed the position of the existing worker party toward the "agrarian reformers." In Germany they expected the triumph of a liberal bourgeois revolution that was merely "the im-

<sup>24</sup> Marx, "Der Kommunismus des 'Rheinischen Beobachters,'" *DBZ*, Sept. 12, 1847, in *Werke*, 4: 193.

<sup>25</sup> Engels, "Das Fest der Nationen in London," in *ibid.*, 2: 612-13.

<sup>26</sup> Engels, "Die Kommunisten und Karl Heinzen," *DBZ*, Oct. 3, 1847, in *ibid.*, 4: 313.

<sup>27</sup> Engels, "Grundsätze des Kommunismus," in *ibid.*, 372-73.



mediate prelude to a proletarian revolution." In general communists everywhere backed every "revolutionary movement against the existing social and political conditions."<sup>28</sup>

In the revolutions of 1848, the only revolutions in which Marx and Engels ever participated directly (they felt that their "time" had come), they avowedly were interested mainly in Germany. They therefore composed and printed in short form for easy distribution a special set of "Demands of the Communist Party in Germany." The "Demands," drafted in Paris on the eve of the return of the two men to Germany in 1848, illustrate the fact that they hoped to mobilize agrarian support for their revolutionary purposes. Four of the seventeen brief demands were distinctly agrarian. All feudal obligations that hitherto had oppressed the country population were to be abolished without compensation. All princely and feudal property in land, mines, and the like were to become state property. The state would introduce large-scale farming along scientific lines on such lands, for the benefit of everyone (*Gemeinheit*). All peasant mortgages were to be transferred to the state, to which the peasants would pay interest in the form of a tax. In areas where tenant farming prevailed, rent and other obligations were to be paid to the state, again in the guise of taxes. "Consumption" on the part of landowners who did not till the land themselves was denounced as a "pure abuse." The above measures, according to the "Demands," were aimed at reducing the "public and other burdens of the peasants and tenant farmers without hurting production and without reducing the sums needed by the state." If the "Demands" as a whole were realized, "the millions who previously had been exploited by a small number would gain their rights and the power that rightfully was theirs as the producers of all wealth."<sup>29</sup> It must be pointed out that there were no similar demands for the confiscation by the state of rental properties, mortgages, and business establishments in the cities—aside from the banks.

When Marx and Engels returned to Germany in 1848 to operate out of Cologne in their native Prussian Rhineland they were supremely confident. Yet they initially had only a few followers already in Germany. They were able to bring with them several hundred additional Germans who were living in exile. These more or less informed and enthusiastic followers were dispatched to different areas in Germany to get something started. Since the proletariat was to provide the correct initiative for the peasants and the broad democratic masses in general, Marx and Engels first tried to get control of the countless worker societies that sprang up everywhere. The attempt failed. Even in Cologne Andreas Gottschalk, supposedly a communist but one who soon showed undesirable devia-

<sup>28</sup> "Manifest der Kommunistischen Partei," in *ibid.*, 491–92.

<sup>29</sup> Marx and Engels, "Forderungen der Kommunistischen Partei in Deutschland," in *ibid.*, 5: 3–5.

tions in tactics, initially became the adored leader of the Worker Society that soon counted over five thousand members. But Marx and Engels were not discouraged. Judging from the precedents set by the great French Revolution Marx and Engels assumed that the high revolutionary tide of March 1848 would be followed by a succession of waves toward the left. Events would open the field for those who acted with decision and comprehended the forces of history.

The key necessity was a daily newspaper. Throughout Marx's and Engels's lives the control over a party "organ" remained a paramount consideration—as a vehicle through which they could reach the wider public, denounce existing governments and the halfheartedness or errors of other parties, and offer daily tactical guidelines for their followers in shifting situations. Their *Neue Rheinische Zeitung* began appearing on June 1, 1848, under the editorial "dictatorship" of Marx and with a staff composed mainly of communists. As the subtitle of the paper, "Organ der Demokratie," suggested, the *NRZ* was professedly democratic. The communist direction of the "Organ" was never specifically avowed, nor was it allowed to surface clearly in the columns.<sup>30</sup>

Beyond that, Marx was personally active in the Cologne Democratic Society. The horizons became broader when Gottschalk was arrested early in July. The Marxian influence then became pronounced in the Cologne Worker Society and in its newspaper, the *ZAV*, which appeared twice a week and on one occasion was rated an "essential power" because it circulated between 1,400 and 1,800 copies.<sup>31</sup> Two communist associates of Marx and Engels, Joseph Moll, who replaced Gottschalk as president, and Karl Schapper, whose abilities as popular leaders had been demonstrated earlier in London, thereafter dominated the proceedings of the Worker Society.

The two societies, together with another related group, sponsored a Democratic Congress in Cologne on August 13 and 14. They invited all similar societies in the Rhine Province and Westphalia to send delegates in order to coordinate policy and create the framework of a wider party. The influence of Marx was certainly in the picture when the Congress recommended a direct, oral propaganda campaign in the rural areas. Emissaries were to go to the peasants, and popular meetings were to be held. The countryside, it was argued, was more open to "democracy" than were the cities. The peasants, reacting against taxes, feudal burdens, and bureaucratic supervision, had become essentially radical.

<sup>30</sup> Engels stated in retrospect that the *NRZ* could not inscribe its "proletarian character . . . on [its] banner." Had they done so, their paper would have been a mere "small, obscure local newspaper," preaching to a "small sect" instead of to a "large active party." Since they were dealing with "despicable" opponents the tone of the paper was anything but "solemn, earnest or inspired." See Engels, "Marx und die 'Neue Rheinische Zeitung' 1848–1849," in *ibid.*, 21: 18–20.

<sup>31</sup> *Zeitung des Arbeiter-Vereines zu Köln. Freiheit, Brüderlichkeit, Arbeit* (Cologne, 1848–49).

They could be won completely for "democracy" if they saw that the "democrats" took up their cause.<sup>32</sup>

In a subsequent committee meeting of the Worker Society the subject "How is the proletariat on the land to be helped?" appeared as the first item on the agenda. The greatest evil existed among them, and it multiplied in geometric proportions.<sup>33</sup> The following session then offered solutions to the problem in terms that resembled the "Demands of the Communist Party." The *ZAV* of September 7 reported that members of the Worker Society had assumed the duty of going into the country to make contact with peasants and factory workers there and to create and maintain regular ties with societies in rural villages. The paper thereafter urged city workers to establish similar links "everywhere with their brothers on the land."<sup>34</sup> The *ZAV* then announced the first brilliant results of the new tactic. Several members had gone to Worringen, a short distance north of Cologne, to discuss political and social conditions with the rural population. They discovered that the "peasants understood very well where the shoe pinched them"; they had the "courage and power" to improve their oppressed lot. The "revolutionary strength of Germany" lay in the peasant and worker classes. If they united they would soon be freed of feudal burdens and the pressures of the usurer and capital. A society of forty members was formed in Worringen on the same day. During this period the *ZAV*, apparently in concert with the *NRZ*, also gave much space in its limited columns to reports of agrarian unrest and protest in scattered areas of Germany.

The success at Worringen was exploited to the utmost. A big popular meeting held there on September 17, 1848, allegedly attended by about ten thousand people, was often referred to as the first communist rally in Germany. Every effort had been made to get a big peasant and worker turnout. It was reported, erroneously, that even Marx appeared on the scene to promote the movement. The local officials charged that a handbill (*Zettel*) with the seventeen communist "Demands" was being distributed. Karl Schapper presided at the rally of September 17, and Friedrich Engels acted as secretary. The "blood-red flag" stood unfurled on one side; the popular German national colors of 1848-49, the black, red, and gold, on the other. The crowd unanimously acclaimed the "democratic-social red republic," and speakers from every locality were recognized, including "Henri [*sic*] Brisbane," the "recognized editor of the democratic-socialist New-York Tribune." The "greatest calm, order and harmony" characterized the affair,<sup>35</sup> a statement that was included in

<sup>32</sup> *NRZ*, Sept. 13, 1848. Curiously, the report was brief and a month late. It was printed only after questions were raised regarding this silence on the part of the "Organ of Democracy."

<sup>33</sup> "Comité-Sitzung vom 21. August 1848," *ZAV*, Aug. 27, 1848.

<sup>34</sup> *Ibid.*, Aug. 31, Sept. 21, 1848.

<sup>35</sup> *Ibid.*, Sept. 21, Oct. 5, 1848.

many reports of popular meetings. But several police spies were detected and unceremoniously pitched into the Rhine, then mercifully pulled out before they drowned.

At the time of the Worringen triumph the committee of the Worker Society in Cologne proposed a second great worker-peasant rally to be held at "Wesslingen" on September 30. The selection of Wesslingen offered a special fillip because the worker emissaries had organized a society there in the face of well-publicized, hostile interference from local officials. Many people, especially in the countryside, wanted to hear the "human doctrine of democracy and socialism," Moll observed. Even if the army opposed them the soldiers (mostly peasant lads) had a "human heart and a social mind." The workers should appeal to them also. Moll, however, warned the committee that the war still had to be restricted to the "mouth and pen."<sup>36</sup> Notices in the *NRZ* and the *ZAV* in the first weeks of September also advertised the appearance of a new radical newspaper in Cologne, the *New Cologne Journal for Townsmen, Peasants and Soldiers*, edited by Fritz Beust and Fritz Anneke. The paper reproduced the tone set by the *NRZ*, though on a more plebian scale.<sup>37</sup>

Moll's warning regarding premature action reflected fully the tactics of Marx himself. An uprising in Cologne and in the Rhineland, unless preceded or accompanied by major upheavals elsewhere, was suicidal and would deprive the movement of its base of operations on the Rhine. Yet, in anticipation of decisive news from Berlin, where a major ministerial and revolutionary crisis was expected, Cologne constructed barricades during the night of September 25-26. They were left standing without defenders when the wires from Berlin did not carry encouraging news.<sup>38</sup> The Prussian authorities nevertheless imposed martial law on Cologne for several days—the "rule of the saber." This involved a temporary suspension of the *NRZ*, *ZAV*, and several other newspapers. A second Democratic Congress in Cologne, scheduled to meet when the crisis came, never held any formal sessions. The popular rally that was to have taken place in Wesslingen about a week later was also canceled. The program in the countryside was interrupted.

During the same weeks of September Marx likewise had expected a revolutionary involvement of the peasantry when the populace in Frankfurt a.M. turned to rioting and the barricades. This had occurred when

<sup>36</sup> "Comité-Sitzung vom 21. September 1848," *ZAV*, Oct. 5, 1848. See also *ZAV*, Sept. 19; *NRZ*, Sept. 12. Even with the help of German scholars in the Rhineland I have been unable to locate "Wesslingen," which apparently lies to the south of Cologne. It has been suggested that "Wesseling" was meant, though the spelling "error," repeated several times, appears a little gross and unusual for the *ZAV*. Marx's *NRZ* (Sept. 12) called the locality "Wesselingen" instead of "Wesslingen."

<sup>37</sup> The German title was *Neue Kölnische Zeitung. Für Bürger, Bauern und Soldaten*. In the last number of the *NRZ*, May 19, 1849, Marx in a sense designated the *NKZ* as the heir of the *NRZ*. The former, accordingly, soon adopted the subtitle of the *NRZ*, "Organ der Demokratie."

<sup>38</sup> See Marx's account, "Die 'Kölnische Revolution,'" *NRZ*, Oct. 13, 1848.

the Frankfurt Parliament reluctantly sanctioned the Armistice of Malmö, which seemed to represent a Prussian betrayal of ardent German national claims to Schleswig-Holstein in favor of Denmark. When the conservative Prussian delegate, Prince Felix von Licknowsky, was torn to pieces by the "enraged mass" in Frankfurt, the *NRZ* gave credit to the peasants who had hurried to the city to do their part for this *Lynchjustiz*, as a "respectable expression of the popular will."<sup>39</sup>

The *NRZ* undoubtedly lapsed into some wishful reporting regarding the actual or destined role of the peasantry. News of peasant revolts (as had occurred early in 1848), in any event, might provide the signal for action among peasants who were already restive. The "honor of Germany" has been defended by workers and peasants from the area, the paper reported. Peasants from countless localities were hurrying to the aid of the barricade fighters in Frankfurt. The revolutionary mood in the Odenwald, Nassau, and Electoral Hesse would block the arrival of additional troops. If the revolt held its own for only a day, the entire surrounding countryside would be in arms; the soldiers would be too weak to suppress the movement. And who would swear by the peasants in the Rhineland, the *NRZ* asked rhetorically; the peasants easily could block the movement of troops (probably Prussian) on the Rhine.<sup>40</sup>

After reports from Frankfurt confirmed the failure of the insurrection the *NRZ* predicted that the revolt was not crushed. The "raging peasants" would not simply put down their arms. The storm that had gathered could be diverted against six to eight princely residences and hundreds of manors. The peasant war that had flared briefly earlier in 1848 was far from being finished.<sup>41</sup> There were only scattered and elusive reports to confirm this expectation.

The revolutionary insurrection in Vienna shortly thereafter produced similar "specters" of peasant involvement. The *NRZ* apparently assumed that the Austrian peasants, freed from *Robot* earlier in the revolution, would rush to defend their gains, lest they be rescinded if Vienna fell. So the paper reported that the country people were streaming in from all directions, even from distant Styria and Tyrol. Peasants from Upper Austria gave assurances that the countryside was ready to respond to a call from Vienna. At a late point in the game 156 Tyrolese sharpshooters, all "intrepid mountaineers," reportedly fought their way into the city to aid the besieged.<sup>42</sup> But whatever peasants arrived, or failed to appear because they did not get the "call," the Croat army of General Jellačić and the mixed units of Prince Windischgrätz successfully invested and stormed Vienna.

<sup>39</sup> "Beilage," *ibid.*, Sept. 13, 1848. In subsequent days the paper offered a somewhat different account of the event.

<sup>40</sup> *Ibid.*, Sept. 21, 1848.

<sup>41</sup> *NRZ*, Sept. 21, 1848.

<sup>42</sup> *Ibid.*, Oct. 12, 14; "Zweite Ausgabe," *ibid.*, Oct. 29, 1848.

During 1848 Marx also repeatedly turned to the question of the liberation of Prussian peasants from feudal burdens and prerogatives. This took the form of denunciations of the machinations of reactionary forces as well as of the liberal elements in the Prussian constituent assembly for failing to act decisively in dealing with agrarian questions.<sup>43</sup> When the Prussian king finally dissolved the assembly and granted his own constitution on December 5, 1848, Marx announced that the action of the king did not free the peasant without compensation. It was the duty of democratic and "rustic" societies to make this clear to the peasants in the Rhineland and Westphalia. If that were done soon, the counterrevolution would face a "phalanx."<sup>44</sup>

AS THE REACTION gained ground in 1848–49 without provoking a corresponding intensification of the revolutionary spirit, Marx repeatedly looked to Paris for another "crowing of the Gallic cock" to herald the dawn of a new and more radical round of revolutions. In this connection Marx expected the peasants in France, that vast majority which had rejected the revolution in the April elections of 1848 and again in the selection of Louis Napoleon as president of the Second Republic, to enter the picture in a positive sense. Marx's imagination was engaged by the revolutionary possibilities of the "billion" (the *Milliarde*, somewhat less than a billion francs) that had been paid in 1825 to the émigrés of the great French Revolution to compensate them for their lost properties. Since a billion was an almost astronomical figure in that age, a demand for the "return of the billion" was sufficiently lofty to become the rallying cry of a new revolution.

After first noting that the Parisian masses had demanded the repayment of the billion (plus three per cent interest since 1825) in the spring months of 1848, the *NRZ* commented favorably on the "precious tactic" adopted by the "Mountain," the leftist opposition to Louis Napoleon, when it sought to gain a peasant following by taking up the same cry. The call for the "repayment of the billion" represented a step on the tail of the "reactionary snake."<sup>45</sup>

On March 11 Marx wrote a leading article on "Die Milliarde" and its revolutionary impact on the peasants. Petitions demanding a repayment of the "billion," plus the interest, were drafted nearly everywhere in France and would soon adorn the walls in all communes, according to Marx. The subject had become the "flesh and blood" of the peasants, as they realized now that their formal ownership of land made them vassals of the capitalist. The "billion" was the first revolutionary measure

<sup>43</sup> *NRZ*, June 25, July 30, Sept. 14, 1848, in *Werke*, 5: 106–07, 309–14, 402.

<sup>44</sup> *NRZ*, Dec. 17, 1848.

<sup>45</sup> *Ibid.*, Feb. 14, 23, Mar. 6, 1849.



that “hurled the peasants into the revolution.” With this “dowry” the “democrats” would win the coming elections.<sup>46</sup>

After recognizing the supposed revolutionary effect of the “billion” on the French peasants, Marx began the search for a “Prussian billion.” To keep the figures within reasonably credible limits he always converted the Prussian *Thaler* (worth about four francs) into francs. Prussia also had her “robber barons” who now put on a “bourgeois” coat and were joining the “Society to Protect the Property of All Classes.” Even the free landowning peasants in the Rhine Province were being threatened with a new scheme of taxation under which they would pay four or five times as much as would be collected in the eastern provinces.<sup>47</sup>

This was followed by another article, “Also a Billion,” that concentrated on the Prussian province of Silesia, where the feudal nobility were then requesting compensation for the rights and benefits they had enjoyed hitherto at the expense of the agrarian population. The “little man” was to be robbed again! But the mood of the peasants had changed, as was evident in thousands of rural villages where peasants were starting to figure up how much they had been robbed during the preceding thirty years when the abolition of serfdom during the Prussian reform era had not been carried out conclusively. The *NRZ*, not surprisingly, asserted that a “billion” (francs, not *Thaler*) was in the picture. The next revolution would bring the “practical realization” of this sum.<sup>48</sup> Compensation to rectify the inequitable tax burden of many years was also involved. The peasants knew enough about “natural history” to know how to “bleed the bloodsuckers.”<sup>49</sup>

A series of articles on the “Silesian Billion” (*Die schlesische Milliarde*), by Wilhelm Wolff (*Lupus*), apparently with some editorial emendations by Marx, appeared in the following issues. Wolff, the son of a Silesian serf, was in a position to add a few vivid details from his own personal recollections.<sup>50</sup> He gave a detailed account of the various forms of “robbery” perpetrated against the agrarian population, together with an estimate of the sums involved. The total exceeded a “billion.” Other “billions” apparently could be certified also, because the situation in Silesia was typical of conditions everywhere in Prussia (except in the Rhine Province) and in Germany at large. The peasants had the right to demand a return of the “billions.” But this would not happen under the existing governments. Only complete destruction of the established system would help. Mean-

<sup>46</sup> Marx, “Die Milliarde,” *NRZ*, Mar. 16, 1849, in *Werke*, 6: 353–56.

<sup>47</sup> “Die Preussische Milliarde,” *NRZ*, Mar. 17, 1849.

<sup>48</sup> “Auch eine Milliarde,” *ibid.*, Mar. 22, 1849.

<sup>49</sup> *NRZ*, Mar. 24, 1849.

<sup>50</sup> Wolff, a member of the recently dissolved Communist League, was on the editorial staff of the *NRZ*. Energetic and fearless, Wolff was an excellent disciple with enough intelligence to grasp the essential views and tactics of Marx without confusing the issue with mental reservations. Marx ultimately dedicated the first volume of *Kapital* to this resolute follower.

while the *NRZ* did not lose sight of the French “billion” and its expected revolutionary implications.<sup>51</sup>

The agitation involving the “billions” ultimately did not lead to marked revolutionary revival, either in France or in the German world. Reprints of the “Silesian Billion” apparently were distributed among the peasants, and the *NRZ* had the satisfaction of reporting that a Count Renard (who had been mentioned as one of the beneficiaries of the “robbery”) indignantly informed the lower house of the Prussian Parliament that the “tale of the Silesian billion has affected the rural population of Silesia in an unhealthy manner.”<sup>52</sup>

Simultaneously the *NRZ* noted that a petition (which would amass seventy thousand signatures) circulated by the agricultural proletariat in Mecklenburg was asking for a “safe and free existence.” Hunger and bitterness were intense; guns and scythes were available in no small numbers. Although Prussian troops had entered the country the Junkers were in danger. A report from Silesia spoke of the formation of a society of farm workers. The report advised the “social-democrats” to intercede “in an active and inciting manner.”<sup>53</sup>

The next, and last, revolutionary storm in 1849, however, did not open with an agrarian overture. The storm resulted from an outburst of German national frustration produced by Frederick William IV’s rejection of the imperial crown of a liberal, united Germany offered by the popular Frankfurt Parliament. If agrarian distress and the lure of the “billion” played a role, it remained subsidiary. The May 1849 crisis led to the expulsion of Marx from Prussian soil. He left Germany behind within the next weeks and went to Paris, still hoping that an expected eruption in France would reverse the triumph of reaction and the return to order. As late as August 1849, just before he went to London, Marx still wrote hopefully of the anger that prevailed among French peasants.<sup>54</sup>

If Marx and Engels frequently groaned over the “stupidity” of the French as well as other peasants (the “barbarian race”), they never dismissed the peasants and the agrarian question as a significant factor in any revolutionary equation wherever the “barbarians” represented the majority. After the failures and disappointments of 1848–49, Engels wrote about the “Peasant War in Germany” in 1852, which reminded the Germans that they also had a revolutionary tradition.<sup>55</sup> Marx published “The Class Struggles in France,” generally regarded as a classic example of the application of his theory of history to recent revolutionary events. Such critical examinations also served as guidelines for the future. Marx

<sup>51</sup> *NRZ*, Mar. 27, 29, Apr. 5, 12, 13, 14, 17, 22, 24, 25, 1849.

<sup>52</sup> “From Berlin, April 19,” *ibid.*, Apr. 22, 1849.

<sup>53</sup> “Aus Mecklenburg,” “Aus Schlesien,” *ibid.*, Mar. 28, 1849.

<sup>54</sup> Marx to Engels, Aug. 17, 1849, in *Briefwechsel*, 1: 111–13.

<sup>55</sup> Engels, “Der deutsche Bauernkrieg,” *Revue*, in *Werke*, 7: 329.

asserted that the French proletariat could not have moved forward until the mass of the nation, "the peasants and the petty bourgeoisie," rebelled against the "rule of capital," and, out of necessity, turned to the proletariat as their "advance guard," or leaders (*Vorkämpfer*).<sup>56</sup> France had approached that point in 1849 when a "significant part of the peasants and the provinces was revolutionized" in response to the promise of the "red party" ("the coalesced democratic party") that offered them the repayment of the "billion," the regulation of mortgages, and the abolition of usury. Only the fall of capitalism and the creation of an anticapitalist, a proletarian, government—"the social-democratic, the red republic, that is the dictatorship of [the peasant's] ally"—could end the "peasant's misery and social degradation."<sup>57</sup> Engels followed a similar line whenever he touched on the revolutionary potential of the German peasant and the agrarian question generally in his "German Imperial Constitutional Campaign" and the series of journalistic articles on the German revolution that appeared under Marx's name in an American newspaper.<sup>58</sup>

The tactical guidelines issued by the revived and again secret Communist League spoke of the nearness of another revolution and restated the need for ties with the agrarian population. The "Address of the Central Authority to the League, March, 1850" stressed the tactical exploitation of the supposed hopes and grievances of the agrarian proletariat, the farm workers. The communists would join forces with the petty-bourgeois democratic party that was destined to make the next revolution. That party was very powerful because it included most of the bourgeois city dwellers and counted on the help of the peasants as well as the "land proletariat, so long as the latter has not yet found a support among the independent proletariat of the cities." The revolution would lead to the creation of a democratic republic in which the democrats would exercise power "for a moment." They would try to call a halt to

<sup>56</sup> Marx, "Die Klassenkämpfe in Frankreich 1848 bis 1850," in *ibid.*, 21. In a later appraisal of French developments Marx again asserted that peasant and proletarian had one thing in common: both were dehumanized and exploited by the same capitalists. Because of their peculiar circumstances, however, the peasants represented no more than the "simple addition of equal quantities," like a "sack of potatoes." They were mere "troglodytes." "They therefore find their natural allies and leaders in the city proletariat, whose aim is the overthrow of the bourgeois order." "Der achtzehnte Brumaire des Louis Bonaparte," in *ibid.*, 8: 198–202.

<sup>57</sup> Marx, "Klassenkämpfe," in *ibid.*, 7: 83–84. Late in life Engels reaffirmed the correctness of Marx's theoretical and tactical position. If history proved they were wrong their miscalculation lay in their failure to see that the economic developments in 1848–49 were not yet ripe for the elimination of capitalist production. The latter had still possessed a capacity for vast expansion. Quite in line with their earlier views, Engels expressed the conviction that the German Social Democrats (the big "Marxian" party) would soon win the support of the greater part of the "middle stratum of society"—"the petty bourgeoisie as well as the peasants." See Engels, "Einleitung zu 'Die Klassenkämpfe in Frankreich 1848 bis 1850' (Ausgabe 1895)," in *ibid.*, 516–17, 526.

<sup>58</sup> Engels, "Die deutsche Reichsverfassungskampagne," *Revue*, in *ibid.*, 111–97. The newspaper articles appeared in the New York *Herald Tribune* between October 25, 1851, and October 23, 1852, and were published separately later under the title *Germany: Revolution and Counter Revolution*, originally under Marx's name.

the revolution, but the communists had to insist on making the “revolution permanent”; the communists had to promote added turmoils designed to force the democrats to carry out their “present terroristic phrases.” The communists were to take a direct hand in perpetrating “excesses”—such as a “popular revenge against hated individuals and public buildings.”

The first direct clash between workers and bourgeois democrats would involve the question of the abolition of feudalism.

As in the first French Revolution, the petty bourgeoisie will want to give the feudal lands to the peasants in the form of free property; that means preserving the land proletariat and the creation of a petty-bourgeois peasant class that will travel the same road toward impoverishment and indebtedness to which the French peasant is still committed.

This the workers had to prevent, in line with their own interests and with those of the land proletariat. Feudal lands were to become state property, to be converted into “agricultural colonies” cultivated by the “associated land proletariat.”<sup>59</sup> If the emphasis in this case was on the land proletariat, the ensuing directive focused more on the peasant.

In a report on the situation in Germany, “The Address of the Central Authority to the League of June, 1850” noted the fact that league members had gained a direct influence over some peasant and farm worker societies, in some cases getting them completely in their hands. Similar societies elsewhere (“Saxon, Franconian, Hessian and in Nassau”) were also mainly under the guidance of the league. The “Address” then stressed the value of gaining influence everywhere over worker, gymnastic (*Turnverein*), peasant, farm worker, and other organizations. The report directed league members to organize persons who were useful and reliable in a revolutionary sense but who had not yet grasped the “communist consequences of the present movement” as “second class” league members, to be led by the real *Bund* members. In this way they could exert a strong influence, especially over peasant and gymnastic societies.<sup>60</sup> In speculating on the chances of revolution following another economic crisis Marx wrote in 1856 that the “whole thing in Germany” depended on the possibility of backing the proletarian revolution “by some second edition of the Peasant’s war.” Then the matter would be superb.<sup>61</sup>

Marx and Engels thereafter never lost sight of the fact that the plight of the agrarian population might serve as the basis of an alliance between

<sup>59</sup> Marx and Engels, “Ansprache der Zentralbehörde an den Bund vom März 1850,” in *Werke*, 7: 244–54.

<sup>60</sup> Marx and Engels, “Ansprache der Zentralbehörde an den Bund vom Juni 1850,” in *ibid.*, 306–12.

<sup>61</sup> Marx to Engels, Apr. 16, 1856, in *Briefwechsel*, 2: 131–32. The quoted phrases were written thus in English. After he learned English Marx frequently used English words, phrases, and occasionally sentences in letters otherwise written in his native German.

the proletariat and the peasant. If less was written on the subject it was because times were relatively unpropitious in an immediate revolutionary sense. During such periods Marx and Engels concentrated more on theory and the preparation of "thick books" as their only "resource."<sup>62</sup>

Interest in tactical ties between the proletariat and the agrarian population nevertheless persisted to the end. Engels, with Marx's encouragement, published new editions of his "German Peasant War" that appeared in 1870 and 1875. In a new introduction Engels stated that the workers in Germany still fell far short of a majority and therefore needed allies. He dismissed the petty bourgeoisie as highly unreliable and denounced the *Lumpenproletariat* as being utterly impossible. The peasant, agrarian masses, in contrast, offered the best hope. Tenant farmers (*Pächter*) could expect salvation only from workers in the form of lower rents. As for the small peasant, he could find relief from the burden of the mortgage and the usurer solely from the proletariat. The most numerous and natural ally of the proletariat was the agricultural day laborer who existed in vast numbers in all of north and east Germany where large estates prevailed. The most urgent task of German workers was to pull these agrarian classes into the "movement." Engels then called attention to the decisions of the Basel Congress of the International Workingmen's Association (IWA) in 1869. It had decreed that the soil was to become "collective national property." So as not to alarm the small peasants, perhaps, Engels explained that this formula was designed primarily for areas where large landownership predominated, as it did in much of Germany and England.<sup>63</sup>

Marx privately informed Engels that the decision of the Basel Congress had been inspired by the General Council of the IWA in London, a body in which Marx's influence was pronounced. The council had suggested the creation of a "Land and Labor League," whereby the "worker party" would sever all ties with the bourgeoisie. Engels replied that the decision had "worked wonders."

<sup>62</sup> The expressions were used by Engels. See Engels to Marx, Feb. 13, 1851, in *ibid.*, 1: 150. Marx's concentration on the "thick books" of *Kapital* in the 1850s and 1860s illustrates the point. Since such "critiques" emphasized theory and historical analysis *Kapital* said little on current tactics, or even tactics in general. It speaks of modern industry annihilating the independent peasant and replacing him with wage labor, thereby engendering the same class antagonisms and desires for social change found in the cities. The farm laborer was similarly enslaved, exploited, and impoverished but possessed a lesser capacity for resistance because of his dispersed situation. Capitalism thereby prepared the way for the "higher synthesis of the future," a union of "agriculture and industry." (*Capital. A Critique of Political Economy* [Modern Library ed.; New York, n.d.] 554-55, 654.) Marx noted that rural violence in England, with its "swing riots," came to the surface almost concurrently with the widely noted uprising in Lyons early in the 1830s (pp. 653-54, 742). *Kapital* touches on such diverse items as the operations of the corvée in the Danubian principalities (p. 260), the thirteen-to-fourteen-hour day among Scotland's agricultural workers (p. 278), the status of agricultural "gangs" (pp. 435-36), the degradation of the peasant into a "serf" (p. 745), and the "swamp of pauperism" in which one foot of the farm worker was stuck (p. 705).

<sup>63</sup> Engels, "Vorbemerkung zu 'Der deutsche Bauernkrieg' (Ausg. 1870 u. 1875)," in *Werke*, 7: 535-37, 542.

People forget that, aside from the large estates, there are various types of peasants: (1) the tenant farmer who does not care whether the soil belongs to the state or to the large property owner; (2) the private owner—first, the large peasant against whose reactionary existence it is possible to incite the day laborers and hired hands; second, the medium peasant who will also be reactionary and who is not very numerous; and third, the debt-ridden small peasant who can be reached through the mortgage. Moreover, one can say that the proletariat for the time being is not interested in questioning the right of small land-ownership.<sup>64</sup>

With this approach Marx and Engels apparently felt that the support of the majority of the rural population could be enlisted even in relatively normal times. They always believed that most could be accomplished within the context of an economic crisis or when the nation was subjected to extraordinary strains, as in time of war.

Late in Engels's life, when many socialist or social-democratic parties with a "Marxian tendency" suddenly and everywhere turned to the countryside, Engels again stated his conviction that "in order to conquer political power, the party first must go from the city to the land [that the party] has to become a power in rural areas."<sup>65</sup> In line with the Marxian view Engels declared that the "development of capitalism irredeemably destroys the landownership of the small peasant."<sup>66</sup> It was the duty of the party to persuade the peasant of this fact. To reassure the peasants Engels stated, "We can only promise that we will not forcibly interfere with the property relationship against their will." Everything "permissible" would be done to make the transition to an "association" (*Genossenschaft*) easier for the peasant. It would not be desirable to wait until "capitalist developments everywhere arrived at their final consequences, until the last craftsman and the last small peasant became the victim of capitalistic exploitation on a large scale."<sup>67</sup> In contrast to the outlook for the small peasants it was possible to offer the land proletariat prospects as bright as those that "beckoned to the industrial worker." Opportunities along this line were excellent in the eastern Prussian areas (*ostelbische*). That was the "decisive field of battle"; a victory there would mean the end of *Junker* rule. Looking at Europe as a whole Engels declared, "From Ireland to Sicily, from Andalusia to Russia and Bulgaria the peasant is a very substantial factor of the population, of production and of political power."<sup>68</sup>

<sup>64</sup> Marx to Engels, Oct. 30, Engels to Marx, Nov. 1, 1869, in *Briefwechsel*, 4: 232, 233.

<sup>65</sup> Engels, "Die Bauernfrage in Frankreich und Deutschland," in *Werke*, 22: 485-86. This article, originally published in two numbers of *Die Neue Zeit*, 1 (1894-95), was translated into Russian in 1903 by Lenin.

<sup>66</sup> "Brief an die Redaktion des 'Vorwärts,'" Nov. 12, 1894, in *Werke*, 22: 480.

<sup>67</sup> "Bauernfrage," 501. See also Engels, "Zur Kritik des sozialdemokratischen Programmentwurfs, 1891," in *ibid.*, 24, where Engels stated that the petty bourgeoisie and the small peasants had no choice but to stand by their "exploiters" or to join the ranks of the workers.

<sup>68</sup> "Bauernfrage," 501.



With reference to Italy Marx early pointed to the agrarian population as a significant force to be enrolled in the battle of national liberation from Austrian rule—an important consideration because Austria, as a bastion of the old order, opposed the course of revolution everywhere. He denounced Mazzini's policy as being fundamentally wrong because it neglected the peasants, for many centuries the "oppressed portion of Italy." The country population there was as "systematically enervated and stupified" as that of Ireland.<sup>69</sup> It took courage to declare that the first step toward Italian independence demanded the "complete emancipation of the peasants and the conversion of their métayer system [*Halbpachtssysteme*] into free bourgeois property."<sup>70</sup> Years later, after Italy was independent and already had a Marxian party, Engels advised the Italian Marxist Filippo Turati that the "*Socialist party*" in Italy was too young and weak to gain an "*immediate victory*." It had to turn to the petty bourgeoisie, a class that faced ruin and that would supply the fighters and leaders for a revolutionary movement. The peasantry would follow them, as "strong and indispensable allies." It was the duty of the Socialists to regard "every revolutionary or progressive movement as a step further in the attainment of their own end." "The victory of the revolutionary movement . . . cannot but strengthen us and place us under more favorable conditions."<sup>71</sup>

<sup>69</sup> The Marx-Engels position in the Irish question requires a special treatise. In general, Marx, through the IWA and otherwise, supported the cause of Irish emancipation as a vital step toward the proletarian triumph of the English worker himself. The English "agricultural oligarchy" as long as it held its "entrenched outpost" in Ireland remained unassailable in England proper. It was easier to attack in Ireland because the issue there was not merely economic but also "national." (Marx to Ludwig Kugelmann, Mar. 28, 1870, in *Letters to Kugelmann* [New York, 1934], 107.) Since the competition of cheap Irish labor in England also depressed the status of the English worker and created divisions in the ranks of labor, the liberation of Ireland would help to unite the English workers against their real foes. (Marx to Meyer and Vogt, Apr. 9, 1870, in *Correspondence*, 288–90.) Engels, after a trip to Ireland, however, found that the Irish became corruptible as soon as they ceased to be peasants and joined the bourgeoisie—this was the case with most peasant nations, but it was especially true of the Irish. The literary representatives of the Irish peasants were thoroughly bourgeois. As a result the Irish regarded the worker movement as "sheer heresy." The Irish peasant was not permitted to know that the "socialist worker" was his "only ally in Europe." (Engels to Marx, Sept. 27, Dec. 4, 1869, in *Briefwechsel*, 4: 228, 254.)

<sup>70</sup> Marx to Engels, Sept. 13, 1851, in *Briefwechsel*, 1: 259–60; Marx to Joseph Weydemeyer, Sept. 11, 1851, in *Correspondence*, 45. The editorial note in the latter (p. 46) incorrectly speaks of "Georg" Weydemeyer.

<sup>71</sup> Engels to Turati, Jan. 26, 1894, in *Correspondence*, 520–22. Marx in the IWA similarly pointed out that workers in different lands had not reached the same degree of development. Hence the "real movement" also expressed itself in differing "theoretical forms" until it ultimately arrived at a "common theoretical program." (Marx to Engels, Mar. 5, 1869, in *Briefwechsel*, 4: 164.) In speaking of different "theoretical forms" here, did Marx set a precedent for later followers who presented temporary tactical programs as theoretically binding? Engels, with reference to party tactics in America, stated that it was "far more important that the movement should spread" than to "start and proceed from the beginning on theoretically correct lines." Their theory was not a "dogma but the exposition of a process of evolution, and the process involves successive phases." (Engels to Florence Kelley Wischniewsky, Dec. 28, 1886, in *Correspondence*, 453.)

THE ATTITUDE OF Marx and Engels toward the Eastern, largely agrarian areas of Europe demands brief but special consideration. Since revolutions in general acted as the "locomotives of history" Marx and Engels also early identified themselves with the cause of agrarian discontent in the overwhelmingly agricultural lands, notably in Poland but gradually in Russia, too. When a struggle for national liberation at the cost of "reactionary" states seemed to call for tapping the revolutionary potential in agrarian discontent, Marx and Engels advocated the cause of agrarian democracy. The liberation of Poland, aside from its direct and local revolutionary benefits, was a necessity also for Germany and the West. The creation of a free Poland would push the frontiers of Russia eastward, eliminating her reactionary pressure on Germany and her capacity to intervene against revolutions everywhere. In 1848-49 and on other occasions Marx and Engels regarded a war against Russia to liberate Poland a revolutionary necessity for Germany. Such a gigantic undertaking would increase domestic strains and would reveal the incompetence and treachery of existing governments and dynasties, thereby hastening a revolutionary drift to the left. A war of such colossal proportions, as Marx and Engels repeatedly stated, could only be conducted and won along revolutionary lines.

The big agricultural lands between the Baltic and the Black Seas, according to Engels, could achieve independence and escape "patriarchal-feudal barbarism" only through an agrarian revolution that transformed the peasants into free landowners. The fight for the national liberation of Poland coincided with the "struggle for agrarian democracy." "The merit of the Poles lay in the fact that they first recognized and announced that the liberation of all Slavic nations could be achieved only through agrarian democracy."<sup>72</sup> Aside from identifying themselves with the cause of Polish agrarian democracy Marx and Engels never indicated what specific role the communists, if any were present, would play in the liberation of such nations via "agrarian democracy." But one can assume that they would not have remained mere spectators. Marx and Engels always insisted that the peasants lacked the ability to assume the revolutionary initiative and would have to look to the proletariat, or possibly the petty bourgeoisie, once Polish peasants gained the freedom French peasants enjoyed, a freedom that subjected them to exploitation by the usurer and by capital in general. In any event, revolutions were likely to progress on an "ascending scale," as the great French Revolution had demonstrated.

Engels developed some doubts regarding the revolutionary utility of the cause of Poland during the next years. The more he studied history, the more it became clear to him that the Poles were a "finished nation," to be used as a "means" only so long as Russia was not projected

<sup>72</sup> "Die Polendebatte in Frankfurt," *NRZ*, Aug. 31, Sept. 3, 1848, in *Werke*, 5: 333, 357.

into an "agrarian revolution." From that moment on, Poland had "absolutely no reasons to exist any longer." Marx, however, displayed a wider perspective. The study of history caused him to come out "decisively direct" in favor of Poland. It was a historic fact, Marx wrote, that the "intensity and the capacity to survive of all revolutions" could be measured quite accurately by their attitude toward Poland. Poland was their "external thermometer."<sup>73</sup> He and Engels, accordingly, greeted the Polish uprising of 1863 with subdued optimism, as an event that might have repercussions in Russia proper.<sup>74</sup>

Marx and Engels, according to the *Briefwechsel* and other sources, did not neglect the revolutionary possibilities, agrarian and otherwise, in Russia. In expectation of a new revolution in Germany in 1851 Engels stated that the overall European picture would remain bleak unless a peasant revolution occurred in Russia.<sup>75</sup> By 1858 Marx found comfort in the assumption that in Russia "*the revolution has started*"; the Crimean War had hastened this turn of events.<sup>76</sup> In the next year the "movement" continued to progress more in Russia than in all of Europe. The nobility was turning against the tsar; peasants were opposed to the nobility; the Poles refused to be Russian. In the next revolution Russia would "obligingly revolutionize" in concert with the rest of Europe. Alexander II had "spoiled" his chances with the peasants, with the result that the "social" movement was being inaugurated both in the East and the West.<sup>77</sup>

Marx thereafter experienced a somewhat ironic pleasure in knowing that the early Russian translation of his *Kapital* enjoyed a wider sale than the original German edition had. A variety of considerations in the 1870s caused Marx to concentrate increasingly on studies of the Russian language and Russian affairs, to the point where he professed a familiarity with many of the native sources. So in 1876 when Marx, as he so often did, asked his partner to assume a major burden, Engels complained somewhat whimsically, "You can lie in a warm bed, study Russian agrarian questions in particular and ground-rent in general and nothing interrupts you."<sup>78</sup> Russia's experience with temporary military reverses at the hands of the Turks in 1877 was seen by Marx as a "new turning point" in

<sup>73</sup> Engels to Marx, May 23, 1851; Marx to Engels, Dec. 2, 1856, in *Briefwechsel*, 1: 157, 204-08. Marx and Engels made a point of participating in most Polish happenings—such as anniversaries of the 1830 and 1846 (Cracow) revolts as well as Polish rallies in general. This served as a common ground on which various democratic, radical, liberal, and even conservative elements could meet.

<sup>74</sup> Marx to Engels, Feb. 13, 1863, in *ibid.*, 3: 126-27, 128-29.

<sup>75</sup> Engels to Marx, May 23, 1851, in *ibid.*, 2: 204-05.

<sup>76</sup> Marx to Engels, Oct. 8, 1858, in *ibid.*, 2: 341-43. Marx, with perhaps the French Revolutionary parallel in mind, stated that he regarded the assembling of the "Notables" in St. Petersburg as signaling the start. On the Continent revolution was "imminent" and would assume at once a socialist character.

<sup>77</sup> Marx to Engels, Dec. 13, 1859, Jan. 11, 1860, in *ibid.*, 2: 448-49, 452-53.

<sup>78</sup> Engels to Marx, May 28, 1876, in *ibid.*, 4: 436-38.

European history. Russia was on the eve of an "upheaval." "All sections of Russian society are in complete disintegration economically, morally and intellectually," Marx asserted. "This time the revolution will begin in the East, hitherto the unbroken bulwark and reserve army of counter-revolution."<sup>79</sup>

In view of the revolutionary prospects in Russia Marx appeared reluctant to discourage certain otherwise sympathetic Russian ideologists who hoped that the Russians might find a different "path of development," growing out of a transformation of the "village commune" into a more advanced form and thereby escaping the tortures of the customary capitalist developments. Marx here declared "straight to the point" that his "historical sketch of the genesis of capitalism in Western Europe" need not have universal application. He cited an example from Roman history to illustrate the point. But he then concluded that, if Russia proceeded along existing capitalist lines, she would "experience its pitiless laws like other profane peoples."<sup>80</sup> As in parts of the *Manifesto*, which also was professedly outspoken, Marx appears to evade the essential question. In a preface to the Russian edition of the *Communist Manifesto* of 1882 he added, "If the Russian revolution becomes the signal for the workers' revolution in the West, so that the one supplements the other, then the present form of land-ownership in Russia may be the starting point of a historical development."<sup>81</sup> The statement is guarded with an "if" and a "may."

After Marx's death Engels was a little more explicit. He was pleased to find that there was a "party among the youth of Russia that frankly and without ambiguity accepts the great economic and historic theories of Marx." While professing a limited knowledge of Russian affairs Engels stated that the revolution "*may* break out there any day." The "historic theory of Marx" was the "fundamental condition of all *reasoned* and consistent revolutionary tactics; to discover these tactics one has only to apply the theory to the economic and political conditions of the country in question." Russia was a "charged mine" that needed only a "fuse to be laid to it." It did not matter how the revolution started or who lit the fuse. In a country "where all these contradictions are violently held together by an unexampled despotism . . . there, when 1789 has once

<sup>79</sup> Marx to Sorge, Sept. 27, 1877, in *Correspondence*, 348-49.

<sup>80</sup> Marx to the editor of the *Otyecstvenniye Zapisky*, end of 1877, in *ibid.*, 552-55.

<sup>81</sup> Quoted in *Correspondence*, 355n.; see also *Werke*, 4: 576. A few years later, in taking note of other special developments in Russia, Marx wrote that the introduction of railroads in countries with limited capitalist developments "accelerated the social and political disintegration," by the creation of a "capitalistic superstructure in dimensions altogether disproportionate to the bulk of the social body, carrying on the great work of production in traditional modes." Marx to Danielson, Apr. 10, 1879, in *Correspondence*, 359. As noted earlier in other connections, Marx here again asserts that the pangs of the Industrial Revolution were experienced more acutely in the less advanced states.

been launched, 1793 will not be long in following.”<sup>82</sup> The agrarian question was one of the sources of such contradictions.

In 1893 Engels returned to the question of whether the “Russian commune might not be the starting point of a new social development.” Had the West been able to upset the “capitalistic regime some ten or twenty years ago,” he asserted, “there might have been time yet for Russia to cut short the tendency of her own evolution toward capitalism.” As it was, Russia, “being the *last* country seized upon by the capitalist *grande industrie*, and at the same time the country with by far the *largest peasant population*,” she experienced the acute “*bouleversement* caused by this economic change.” Meanwhile the commune was fading away. In a later letter to Danielson, Engels added,

No more in Russia than anywhere else would it have been possible to develop a higher social form out of primitive agrarian communism unless—that higher form was *already in existence* in another country, so as to serve as a model. That higher form being, wherever it is historically possible, the necessary consequence of the capitalistic form of production and of the social dualistic antagonism created by it, it could not be developed directly out of the agrarian commune, unless in imitation of an example already in existence somewhere else.<sup>83</sup>

That may have been the more explicit expression of Marx’s own position.

THE LIVES AND DEEDS of Marx and Engels indicate that they were not unduly hobbled by the apparent implications of their own theories that stressed the revolutionary role of the advanced industrial states. Marx and Engels were generally ready to side with any groups against an existing government. In predominantly agricultural countries this meant an appeal to the rural masses. As champions of democracy, that broad, popular aspiration that sought salvation in the rule of the people, Marx and Engels were confident that the communists who represented the proletariat and comprehended the laws of historical development would capture the initiative in any such democratic front. That possibly was enhanced by the characteristics with which Marx and Engels endowed their followers—energy, decisiveness, a readiness to make tactical adjustments to the requirements of the hour, and a certain “communist pride of infallibility” (*Kommunistenstolz der Unfehlbarkeit*) that Marx once ascribed to the communists.<sup>84</sup>

The question of whether Lenin operated along truly Marxian lines in 1917 cannot be answered solely on the basis of an appeal to theory.

<sup>82</sup> Engels to Vera Zasulich, Apr. 23, 1885, in *Correspondence*, 436–38.

<sup>83</sup> Engels to Danielson, Feb. 24, Oct. 17, 1893, in *ibid.*, 508–10, 515.

<sup>84</sup> See Marx to Engels, Aug. 25, 1851, in *Briefwechsel*, 1: 244–51.

One must take into account the actions, tactics, and expectations of Marx and Engels in reasonably comparable circumstances. If Lenin appealed to the broad rural masses he certainly did not noticeably deviate from the tactics repeatedly advocated by the master.

As for the position of Russia in 1917, Was the latter so much more retarded industrially than the Germany of 1848 where Marx and Engels had expected an early communist triumph? Was Russia not also affected by the special miseries that afflicted a backward country because of the competition of the industrially advanced states and the belated arrival of capitalist developments? Did the Russians not experience the suffering, confusion, demoralization, and decline of confidence in the ruling classes that accompany a protracted, disappointing war—all major considerations that Marx and Engels counted on, as when they advocated war in 1848–49 or when they speculated on the revolutionary consequences of war on other occasions? Did the revolution in Russia not occur in a “more advanced stage of civilization” than was the case with earlier revolutions? The Russians perhaps also possessed “minds” with a greater capacity for “generalizing.”

It appears, then, that if he had neglected to see the opportunity to act and if he had not acted as he had—with energy, decision, and a total disregard of “boring scruples”—Lenin would have failed Marx. Perhaps the same can be said for communist leaders in other lands (like China) where the purely “theoretical” circumstances favoring a communist seizure of power were even less auspicious. Marx and Engels, of course, at no point exactly advocated the distribution of land to the peasants, as Lenin did in 1917. But in certain revolutionary situations, as when they supported national liberation movements against undesirable governments (notably in Poland but also in Ireland and to a certain extent in Italy), they advocated an “agrarian democracy” that apparently was based on the acquisition of the land by the peasants. In Germany Marx and Engels were ready to give assurances to the small peasants that the revolution would not lead to the liquidation of small private ownership. The gap that separated them from Lenin’s tactics in 1917 was not too formidable. Since Marx and Engels continually stressed the importance of getting a revolution started and the need to consolidate a revolutionary position, it might even be presumptuous to assume that, given the same historical circumstances, they would have hesitated to take the step that Lenin took. They certainly always paid tribute to the virtue of “audacity.”



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## Parliament and Society in Early Stuart England: The Legacy of Wallace Notestein

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A Review Article by THEODORE K. RABB

WALLACE NOTESTEIN. *The House of Commons 1604-1610*. New Haven: Yale University Press. 1971. Pp. xiii, 598. \$18.50.

ROBERT ZALLER. *The Parliament of 1621: A Study in Constitutional Conflict*. Berkeley and Los Angeles: University of California Press. 1971. Pp. xi, 242. \$9.00.

ROBERT E. RUGH. *The Parliament of 1624: Politics and Foreign Policy*. (Harvard Historical Studies, vol. 87.) Cambridge, Mass.: Harvard University Press. 1971. Pp. x, 434. \$15.00.

ALMOST FIFTY YEARS have passed since the publication of *The Winning of the Initiative by the House of Commons*, the famous essay by Wallace Notestein that opened a new era in studies of parliament before the Civil War.<sup>1</sup> More than twenty years elapsed before much further progress was made, but in the last two decades the monographs have begun to appear that have made the century from 1560 to 1660 the most thoroughly studied in the history of any representative assembly. This outburst of scholarship has, of course, been fueled by interest in the English Civil War, one of whose sides coalesced around parliament. But the immediate inspiration has clearly come from the two pioneers in the field, Notestein and Sir John Neale. They and their students have ransacked archives public and private to extract the last ounce of information about nearly two dozen sessions of the Commons (thirteen under Elizabeth I and nine under James I), and their followers have delved equally deeply into the further assemblies of Charles I and the Interregnum. Both Notestein and Neale, blessed with long life, have lived to see their efforts bear manifold fruit, and now the appearance of Notestein's last work, published posthumously, suggests that it may be time to review the accomplishments of at least the Stuart half of this mountain of scholarship.<sup>2</sup>

From one perspective the importance and uniqueness of developments in the House of Commons during the early seventeenth century have

<sup>1</sup> Wallace Notestein, *The Winning of the Initiative by the House of Commons* (London, 1924).

<sup>2</sup> The chief works of the two pioneers are J. E. Neale's *The Elizabethan House of Commons* (London, 1949) and *Elizabeth and Her Parliaments* (London, 1953, 1957), and the edition by Notestein, Frances Relf, and Hartley Simpson of *Common Debates 1621* (New Haven, 1935).

never been so widely appreciated. The studies sponsored by the International Commission for the History of Representative and Parliamentary Institutions in the last few years have only served to underline the pathbreaking achievements of the few hundred parliamentarians who created the first effective government centered around an elected assembly. And yet within recent Tudor-Stuart historiography this all seems to be taken for granted and attention is focused elsewhere. A century ago S. R. Gardiner, writing the ten-volume work that has remained the standard political history of the years 1603–42, saw the conflict between king and Commons as the essential encounter that gave shape to the period.<sup>3</sup> Gardiner built his narrative around parliamentary sessions and presented the Civil War as the result of the irreconcilable claims of an arrogant monarch and his ambitious subjects. It was within this framework that Notestein wrote his essay on the winning of the initiative. But in the last twenty years or so the gentry controversy has upstaged Parliament, which now appears as no more than the instrument of much more powerful forces.

When Lawrence Stone recently summed up the causes of the English Revolution he placed his emphasis on three elements—the Crown's failure to gain an army or a bureaucracy; the relative rise of the gentry in wealth, status, education, administrative experience, group identity, "and also [in terms] of political self-confidence in the House of Commons"; and the spread of Puritanism.<sup>4</sup> Although Parliament appears low in these rankings, few contemporary Stuart historians would argue with Stone's assessment. Yet perhaps this shift only signifies that we are unable to link ostensible and underlying causes. The appearance of these three books provides a good opportunity to re-examine some of the issues and to place Notestein's legacy in the context of current studies of Stuart England.

THE CENTRAL PROBLEM is the extent to which the stated objectives of historical actors are regarded as sufficient indicators of their motives. Bernard Bailyn has recently stimulated a major re-examination of the American Revolution by suggesting that Locke really did matter to the colonists, that ideological disagreements did impel sober citizens to take up arms. Is a similar reassessment due for seventeenth-century England?

The differences between the two cases are vast, especially since the English of Stuart times had neither theory nor philosophy to guide them. There was no Locke, no Bill of Rights, not even (and this was a major problem) an accepted definition of royal prerogative or the privileges of subjects. In that sense, therefore, one can hardly speak of ideological

<sup>3</sup> S. R. Gardiner, *History of England from the Accession of James I to the Outbreak of Civil War, 1603–1642* (London, 1883–84).

<sup>4</sup> Lawrence Stone, "The English Revolution," in Robert Forster and Jack P. Greene, eds., *Preconditions of Revolution in Early Modern Europe* (Baltimore, 1970), 55–108, especially 96.

origins of the English Civil War. Nonetheless there was undeniably constant recourse to abstract notions of justice, equity, and right. Above all there was a very powerful consciousness of the supremacy of law; and in the revival of Magna Carta—however distorted and misapplied—there was an attempt to set a standard against which political acts could be measured. The question is: how much weight can one give such arguments? Were they genuinely believed, or were they merely cloaks, possibly unconscious, for the assertion of more material aims?

The three books under consideration come down quite decisively on the side of the parliamentarians' sincerity. "A moderate given neither to overstatement nor to enthusiasm," who "had a knack for drawing men along with him in a common pursuit of reason and of English liberty" is the way Notestein sums up the personality and aims of Sir Edwin Sandys, the most prominent exponent of opposition to the Crown from 1604 to 1621. Neither Zaller nor Ruigh would seem to disagree—in Zaller's eyes Sandys exercised "good sense and moderation," and to Ruigh he was one of the moderates.<sup>5</sup> Even Coke, bitter, vengeful, and belligerent though he was, emerges as a devoted adherent of "the reign of the common law," a pursuit that "redeemed an often sordid career." It is perhaps an exaggeration to regard him in 1621 as setting out "to rectify what he considered a dangerous imbalance in the constitution"—hardly a concept Coke would have understood—but it would be difficult to deny that he "always sought . . . to create a bulwark that could protect the subject and his precious legal rights from the inroads of government."<sup>6</sup>

Moderation and legalism are scarcely the terms by which one expects the forerunners of revolutionaries to be described. Rousseau, the social critics of the nineteenth century, even Locke never evoke such restrained epithets. True, the moderation was sometimes abandoned, but only over narrow issues—the punishment of Edward Floyd, who had libeled the king's daughter, occasioned by far the most angry and hysterical outburst in the Commons during James I's reign. And as one reads the debates one becomes convinced that the arguments and proposals that were aired represented exactly the concerns of these serious and determined country gentlemen. Their hearts were on their sleeves, their worries were laid bare, and yet one finds no hint of revolutionary rhetoric.

What was it, then, that stirred the M.P.'s? In the 1604–11 sessions, according to Notestein, there were three basic issues. In descending order of importance they were: those royal prerogatives, such as wardship and purveyance, which were sources of revenue for the king; the union with Scotland; and the regulation of foreign trade. In 1621, as Zaller

<sup>5</sup> Notestein, *Commons 1604–1610*, 7; Zaller, *Parliament of 1621*, 48; Ruigh, *Parliament of 1624*, 244.

<sup>6</sup> Zaller, *Parliament of 1621*, 53, 52.

points out, the problems shifted: to the conduct of foreign policy and of royal ministers, to religion, and to the trade depression. Finally, in 1624, Ruigh sees foreign policy and, once again, ministerial behavior as the only matters of major concern, though it could be argued that this is to miss the considerable significance of the Monopolies Act, which, remarkably enough, is not once mentioned in the book.

The problems that the Jacobean Parliaments struggled with thus centered on the royal government's activities—its financial devices, its foreign policy, the actions of its servants. Religion and trade were recurrent and important, but clearly subsidiary issues. And the standards used for judgment were always custom and law. As Notestein observes, "those who would maintain the rights of Parliament found it prudent to go back and recall the fourteenth century."<sup>7</sup> Although it is true that a conscientious interpretation of the precedents would only have reinforced the king's rights, in one vital regard these gentry were right. For what they wanted was a return to that system of centrifugal authority which, in previous ages, had sustained their power and kept them immune from the interference of a distant central government. That invulnerability was fast crumbling in the face of exactions like wardship and purveyance, of Privy Council interventionism, and of the spreading tentacles of royal courts. Behind the attacks on specific grievances always lay the fundamental worry that the government was becoming too powerful, that it was moving into areas outside its proper sphere, that individual rights and property were threatened, and that the servants of the Crown were acting in haughty indifference to tradition.

The only defense, so it seemed, was an independent parliament, and thus the M.P.'s fought for their privileges with unyielding determination. It was only because they got their way on perhaps the most fundamental right of all, freedom of speech, that the 1624 session became, in the phrase Ruigh quotes, a "*Parlementum Foelix*." This is why the battles over seeming trivia were in fact so crucial to the Commons. If they failed, who would protect the ordinary property owner? Certainly not the courts—they had proved all too fragile. The privileges and independence of Parliament thus came to be synonymous with the independence of all Englishmen in their localities against the encroachments of royal power. Of the three authors, Notestein, the master, perceives this connection best, and he emphasizes the development of new procedures such as the Committee of the Whole House and the changing role of the Speaker as the means whereby the M.P.'s fortified themselves against the Crown. Yet it was in 1621 that the decisive breakthrough came, with the revival of the power of impeachment. Zaller rightly stresses the importance of the proceedings against Francis Bacon, but he does not seem to appreciate

<sup>7</sup> Notestein, *Commons 1604-1610*, 391.

fully that now the Commons grasped a weapon more dangerous than the one used hitherto, the withholding of taxes. This doubling of their armory changed the parliamentarians' relationship with the Crown decisively, and both Cranfield and Charles I were later to regret bitterly their encouragement of the Commons in 1621 and 1624 for immediate ends.<sup>8</sup> Ruigh sees that the winning of the right to discuss foreign policy was a vital advance for the subject, but he, too, fails to set the events of the Parliament of 1624 into their long-term context.

It is at this point that one must return to the basic question: what did these lengthy and often tedious debates have to do with revolution? Few historians study the first half of the seventeenth century without looking ahead to the 1640s; and yet here, in the institution that became the arena for civil war, the future is played down almost into nonexistence. Only when the Puritans come on the scene—usually in support of ambitious but abortive schemes for reform—does one get a sense of a real threat to the established order. Nobody but a Puritan could have made so ominous and enthusiastic a demand as that contained in a manuscript quoted by Notestein, in which M. P.'s were asked to be ready "in behalf of the freedom of the gospel to hazard their estates rather than leave their posterity to perpetual thralldom."<sup>9</sup> This is the stuff of which revolutionaries are made, but somehow the Puritans remain in uncomfortable relationship with the other members of the Commons. What emerges are two distinct sets of aims, held by different members of Parliament. Both groups resisted the king, but the more numerous and visible of the two was essentially conservative, thus creating the paradox that lies at the heart of the problem of linking parliamentary history with the Civil War.

On the one hand stand the gentry, wedded to legalism and moderation, anxious to hold back the incursions of central authority. Alongside them, and mixed in with them, are the Puritans, determined to refashion society. In all three of these books—indeed in all three of the *Parliaments*—the chief actors are the gentry, and the Puritans are little more than peripheral. Is this, perhaps, why so little hint of the 1640s appears? Partly, but the problem goes much deeper. For after all, a huge amount of research has been devoted in recent years to the gentry, primarily with the object of explaining why this conservative class, at the head of a deferential society, launched a revolution. Social, economic, and intellectual analysis has been devoted to just this end, and yet somehow these studies and the studies of Parliament seem to pass each other like ships in a fog—beyond an occasional booming echo, there is no attempt to offer mutual assistance. Since the social history is currently far more common it is not difficult to see how the impression arises these days that

<sup>8</sup> Zaller, *Parliament of 1621*, 24; Ruigh, *Parliament of 1624*, 343–44.

<sup>9</sup> Notestein, *Commons 1604–1610*, 41.

English historians are slighting events in Parliament when they try to explain the Civil War.<sup>10</sup>

This difficulty partakes of a much larger problem, and one that faces historians in all fields—how to connect the growing body of research into social history with the extensive long-standing findings of political history. One body of pioneers in social history, the Sixth Section of the *Ecole Pratique des Hautes Etudes* in Paris, evades the issue by ignoring political history. But in studies of seventeenth-century England the dilemma is nakedly before us. One set of historians clings to the view of the revolution as a political struggle of independent-minded or Puritan gentry versus an arbitrary or overambitious king; the other group stresses the rise of the gentry as a class that had come to terms with changing conditions in the economy, the social structure, land use, and so on, and now made its lunge for political power. The two views are not incompatible, but they have never really merged. As one surveys the field, though, and sees the superb works on both sides of this divide, one must conclude that, if political and social forces are ever to be blended, here is where historians have a golden—perhaps their best—opportunity. The research has been so saturating, the intelligence applied to the many problems so acute, and the striving for synthesis so manifest, especially in recent years, that surely the links must soon begin to appear. Moreover, as the last few paragraphs indicate, some of the links are already implicit in the literature.

ON THE SIDE of parliamentary studies the materials are now almost too abundant—a testimony to the breadth of Notestein's inspiration. The publication of these three books means that there is a volume devoted to every single Parliament of James I's reign.<sup>11</sup> The work of J. N. Ball, Conrad Russell, and Christopher Thompson will be adding to our knowledge of the 1620s in the next few years;<sup>12</sup> and the Long Parliament, already amply investigated, is enjoying a new spate of research. When the Yale Parliamentary Diary project is complete most of the primary sources for the 1620s will be published, too. Few major areas of historical research have been laid so bare, with biographies of most of the leading actors also available. The one grievous lacuna is a biography of the duke of Buckingham, an unpleasant and lifelong project that thus far nobody

<sup>10</sup> An example can be taken from the most important study of English society under the Tudors and Stuarts published in the last decade, Lawrence Stone's *The Crisis of the Aristocracy 1558-1641* (Oxford, 1965). Stone disarmingly sidesteps the problem by saying that his book is a prolegomenon to, and explanation of, political history, but that Parliament is "deliberately excluded" from consideration (p. 8).

<sup>11</sup> The only Jacobean session not covered by these books is that of 1614, which is treated in T. L. Moir, *The Addled Parliament* (Oxford, 1958). Mention should also be made of one other most important study of the early Stuart Commons: D. H. Willson, *Privy Councillors in the House of Commons 1604-1629* (Minneapolis, 1940).

<sup>12</sup> These three scholars are working on Sir John Eliot, John Pym, and the earl of Warwick and his circle, respectively.



has been willing to undertake. Its absence, though, hardly undermines the thorough understanding of the politics of the 1610s and 1620s that has been achieved.

What these three works have done is to add further concrete detail to the picture of gradual estrangement between king and Commons that has been familiar since Gardiner and especially since Notestein's classic essay of 1924. But they are rather different in approach and aim. Notestein proceeds at a leisurely pace, with the sure tread of one who is thoroughly familiar with every inch of territory that he passes. The judgments ring true again and again, the chronology is lucidly laid before us, and in two analytic sections the committee system and the role of the speaker are given definitive treatment. The one subject that is missing is the election of the House, and one could have wished for more detailed treatment of the relations between Commons and Lords. But there can be no doubt that this is a worthy *opus ultimum*—where *The Winning of the Initiative* laid the foundations, this book has constructed an imposing edifice of scholarship.

As a contrast in styles, Ruigh and Zaller could hardly be more dissimilar. Zaller leans heavily on Notestein's seven-volume edition of the 1621 diaries and has sought out few other primary sources. His book is short, spare, and sparkling. He has a fine ear for a phrase, and his assessments of men and situations are quick and shrewd. It is a pleasure to read the book, though one often wonders whether a great deal more does not still need to be discovered. Naturally, too, one has occasional misgivings about the flashes of interpretation—Coke, for example, is perhaps overstressed, and the broader implications of Bacon's fall receive inadequate attention. Yet certainly in style, in the willingness to make general comments about Stuart politics, Zaller upholds Notestein's tradition.

Ruigh's is a much more weighty tome, and sometimes rather heavy going. But there can be little need for further work here. He has examined an enormous range of papers, and in his best section—on elections and the patronage of various groups—he has brought some extremely difficult and scattered research to a most successful conclusion. These three chapters are models of painstaking historical work, and there is nothing like them in the literature on Parliament. In the very close study of day-to-day diplomatic history and its reflections in the Commons, though, one sometimes loses the wood for the trees. This book will be a mine for future scholars, and Notestein would doubtless have been proud of it, but he, too, would probably have wished that the sights could have been raised higher than the spring of 1624 more often.

Nobody can dare to say that research is ever complete, but it seems safe to predict that studies of particular sessions of the House of Commons during James I's reign are unlikely to appear again for at least a generation. Related subjects need further work, to be sure—notably

the House of Lords, which has received adequate attention only from Zaller. But surely now the task is to begin integrating the huge accumulation of political history with the multiplying studies of English society. Did the issues raised in Parliament reflect profound social divisions? Was there a fundamental change in direction between James I's and Charles I's reign or merely an intensification of problems already apparent by the 1620s? Were the non-Puritans revolutionary or reactionary? Were economic issues of any significance? Did England really experience a revolution at mid-century? If we can begin to answer these and similar questions, Notestein's legacy will gain new dimensions and a new importance for all historical research.

HAVING PAID HOMAGE to a distinguished historian whose work is now complete, and whose bequest to the future requires serious attention, I feel I cannot end this essay without responding, in Notestein's name as far as I can, to a questioning of his contribution that appeared in print not many months after he died, too late for him to defend himself. In the course of a discussion of the purposes of parliamentary history from the Middle Ages to the present, Geoffrey Elton has expressed a series of doubts about Notestein's scholarship and interests,<sup>13</sup> and conscience drives me to respond.

Elton's first stricture is an unsubstantiated charge that the edition of the 1621 diaries lacks scholarly rigor and is not easy to use. Yet these volumes, meant for the scholar doing detailed work, not for the casual reader, have proved their usefulness for over thirty years. One cannot dismiss the standards or the comprehensibility of the edition, its cross-references, explanations, and superb index, without extensive documentation.

The second stricture is more general. According to Elton the title (and presumably the contents) of Notestein's famous essay "urge us to seek nothing but the 'growth' of the Commons' independence," a "whiggish" view that has been discredited by the Commons' lack of initiative since the mid-nineteenth century. In fact the essay discusses much besides growing independence; furthermore, whether Whiggish or not, Notestein's interpretation of Stuart times is hardly vitiated by developments since Victoria's reign. And what better organizing principle is there for a history of the Commons under James I and Charles I?

Elton broadens his criticism by asking whether "all that matters" is the ambition of "representatives" to limit the "executive," an emphasis he attributes to "American scholars, who perhaps cannot be expected to know better." But Notestein had no such exclusivist view; he did not

<sup>13</sup> "Studying the History of Parliament," *The British Studies Monitor*, 2 (1971): 4-14. Since it is such a short article I have not given references for each quotation. The arguments I discuss can be found on pages 7-11. I might note that the remaining eight paragraphs of my article were added after the rest had been accepted for publication.

treat the struggle between king and Commons in such anachronistic terms; and his analysis of parliamentary history does not demonstrably owe more to American birth than to bicycle riding.<sup>14</sup>

Elton's third stricture is the obverse of the second. To correct the balance, he says, one must ask "the administrative historian's questions." Harmony, not conflict, was the normal political situation; therefore the main emphasis should be placed on procedural stability. Studies of the early Stuarts err in their conflict-inspired "conviction that only opposition entitles a man to respect," and Notestein is specifically reproofed for his "almost perverse" emphasis on 1621, when no legislation was passed except for two subsidy bills. Elton is looking for "more original scholars," such as his students, to investigate procedure and give us "real hopes of finding out the truth."<sup>15</sup> After all, he complains, "we do not even know for sure how committees were appointed—by whom, at whose nomination" in the seventeenth century.

Despite his admission that "administrative" and "political" should not be separate categories, Elton fails to recognize that Notestein's work is notable precisely because he interwove administration and politics. Indeed he regarded changes in procedure (for instance, the new use of committees) as *the* essential mechanism whereby political initiative was won. *Pace* Elton, specialists do know how committees were appointed, and the best discussion of the subject is included in the forty-page chapter on committees in Notestein's last book. As for one-sidedness, that same work accords nobody more respect than Cecil or Bacon.

Although Notestein's particular blend of procedure and politics might not satisfy Elton, it is misguided to deny that the blend is there. Notestein perceived that procedures, continuity, and harmony are the necessary backdrop—but only the backdrop—against which change should be discerned. Elton's contrasting opinion, implicit in his belief that a Parliament's significance is related to the number of bills it passed, rests on a remarkable view of history. Most historians would surely say, with good reason, that the importance of a session derives not from the quantity of its formal bills but from the content of its bills or proceedings. In 1621 the great business was the revival of impeachment and the ruination of Bacon, neither of which produced a bill. But impeachment and the lack of bills dealt grievous blows to the harmony of government. Elton might argue that there was still much harmony—so there was,

<sup>14</sup> Elton himself pays tribute to the work of an American, Stanford Lehmberg, and one wonders how Elton can seriously believe that just because he lives in England he is likely to have a better understanding of distant parliaments than a Notestein or a McIlwain.

<sup>15</sup> True to his views, Elton regards the discussion of procedure "the most remarkable part of" Neale's work, and Elton heralds Elizabeth Foster's study of the clerk Henry Elsyng as part of the beginnings of great illumination. I am not sure Professor Foster would regard her admirable biography in quite that light, but in any case Elton does not appreciate that she is a student of Notestein, or that the inspiration of her teacher is apparent throughout her research.

and Notestein remarks upon its persistence into 1640 and 1641—<sup>16</sup> but to concentrate on that to the exclusion (or minimization) of conflict and change is to miss the dynamism that gives history its appeal and distinguishes it from other studies. To alter that situation Elton must first alter human nature.

In sum, Notestein's aims and achievements have served as straw men for an ill-founded polemic. He may be open to criticism, but certainly not to these criticisms. Elton could claim that the book on 1604–10 was printed too late to affect his article. Yet most of my comments could be derived from *The Winning of the Initiative*, and in any case the book not only was published more than three months before Elton's article appeared, but was announced the previous year. The least a scholar should be able to expect is that his colleagues not attack his life's work with unseemly haste once he is no longer alive to respond. In this particular case the critic might also have given the reader some inkling as to why most historians of the period would place Notestein among the five or six scholars who have done most during the last half century to shape our understanding of early Stuart political history.

<sup>16</sup> *Initiative*, 25. Paradoxically Elton published an essay in 1970 in which he suggested that the Civil War was perhaps not so "exceptional," because conflict was a part of English life. See "The Unexplained Revolution," *Encounter*, July 1970, pp. 77–81, especially 81.

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## The Transition from Feudalism to Capitalism in Russia: A Major Soviet Historical Controversy

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A Review Article by SAMUEL H. BARON

*Perekhod ot feodalizma k kapitalizmu v Rossii: Materialy vsesoiuznoi diskussii* [The Transition from Feudalism to Capitalism in Russia: Materials from the All-Union Discussion]. Akademiia Nauk SSSR, Nauchnyi soviet "zakonomernosti istoricheskogo razvitiia obshchestva i perekhoda ot odnoi sotsial'no-ekonomicheskoi formatsii k drugoi." Edited by V. I. SHUNKOV *et al.* Moscow: Izdatel'stvo "Nauka." 1969. Pp. 412.

A RANDOM SAMPLING of the views on Soviet historiography of America's Russian specialists would almost surely yield something along the following lines. Soviet historians have zealously mined masses of previously unexploited sources and have made available a wealth of factual data, but their interpretations are often so bizarre that they hardly inspire confidence. This is so partly because these writers frequently have preconceived answers to the problems they investigate, and accordingly their research is one-sided. They seek support for what they wish to prove, exaggerate the significance of such evidence as they find, and ignore or minimize the importance of evidence of a contrary kind. Such failings derive in good part from the heavily ideological character of Soviet historiography. The "classics of Marxism-Leninism" are considered as a kind of scripture in which may be found either the answers *tout court* or strong guidelines to the answers to most questions: hence the extraordinary role of quotations from the "classics" in Soviet historiography and their employment not as hypotheses to be tested but as axioms to be illustrated. The situation is made worse by the imposition of political controls, which bar from many areas the free competition of ideas that might make for self-correction and which tend instead to produce a leaden conformity.<sup>1</sup> For such reasons Soviet

<sup>1</sup> An examination of reviews of Soviet works in the *American Historical Review* and the *Slavic Review* will generally bear out this characterization—but certain important qualifications must be added. American and other Western students recognize that a number of Soviet historians produced significant contributions even in the depths of the Stalin era. In the post-Stalin period, thanks to a somewhat improved climate for historical work, the share of historical production worthy of attention has increased. Works that offer dubious interpretations, moreover, are often partly redeemed by the inclusion of important new material

historiography represents an alien "continent" whose exploration hardly repays the effort, and dialogue between Soviet and Western students of Russian history is well-nigh impossible.<sup>2</sup>

The book under review is of quite extraordinary interest, for it compels a reconsideration of this appraisal, at least in respect to one broad area of historical investigation. In question is the recently published record of a conference held in 1965 to discuss a problem that has bedeviled Soviet historiography for decades—the transition from feudalism to capitalism in Russia.<sup>3</sup> Soviet students of their country's history have long taken it for granted that Russia passed through a feudal epoch, which began as early as the ninth or tenth century and persisted until 1861, when serfdom was abolished and the capitalist era ushered in. Understandably they have considered the investigation of the genesis of capitalism "within the womb of the old order" one of their major tasks.<sup>4</sup> If something like leaden con-

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from the archives. On these matters and on other developments in post-Stalin historiography, see John Keep and Lilianna Brisby, eds., *Contemporary History in the Soviet Mirror* (New York, 1964); Kurt Marko, *Sowjethistoriker zwischen Ideologie und Wissenschaft* (Cologne, 1964); Hans Rogger, "Politics, Ideology, and History in the USSR: The Search for Coexistence," *Soviet Studies*, 16 (1965): 253-75; and Arthur P. Mendel, "Current Soviet Theory of History: New Trends or Old?" *AHR*, 72 (1966-67): 50-73. See also the journal *Kritika* (published three times a year at Cambridge, Mass.), which performs a valuable service by printing long and often penetrating reviews of Soviet historical works.

<sup>2</sup> In contrast to the prevailing view, Fernand Braudel some years ago laid on Western historians the obligation to "follow and follow closely the work of [Soviet] historians" unless they would risk ignoring "an entire 'continent' of history." See the editorial note in *Annales: Économies, Sociétés, Civilisations*, 12 (1957): 127.

<sup>3</sup> The conference, sponsored by the Academy of Sciences' Scientific Council on "the lawfulness [zakonomernost'] of the historical development of society and the transition from one socio-economic stage to another," took place in Moscow on June 2-4, 1965. It was attended by some three hundred historians from all over the USSR. Before the conference began, each received a copy of a report composed by a committee whose members were I. F. Gindin, L. V. Danilova, I. D. Koval'chenko, L. V. Milov, A. P. Novosel'tsev, N. I. Pavlenko, M. K. Rozhkova, and P. G. Ryndziunskii. Pavlenko was the committee's leading spirit and spokesman. The printed record (hereafter *Perekhod*) includes the report, the comments on it offered by thirty-eight of the conferees, a digest of written remarks submitted by persons who, for lack of time, had no opportunity to speak, and a brief set of recommendations developed out of the conference materials. The first published information about the conference was a brief report: Iu. Bromlei, "Izuchenie problemy perekhoda ot feodalizma k kapitalizmu v Rossii" (The Study of the Problem of the Transition from Feudalism to Capitalism in Russia), *Vestnik Akademii Nauk SSSR*, 35 (1965): 114-16. The following year more of the substance of the controversy was publicized through articles by representatives of the contending groups: I. A. Bulygin, E. I. Indova, A. A. Preobrazhenskii, Iu. A. Tikhonov, and S. M. Troitskii, "Nachal'nyi etap genezisa kapitalizma v Rossii" (The Opening Phase of the Genesis of Capitalism in Russia), *Voprosy Istorii*, no. 10 (1966): 65-90; N. I. Pavlenko, "Spornye voprosy genezisa kapitalizma v Rossii" (Controversial Questions on the Genesis of Capitalism in Russia), *ibid.*, no. 11 (1966): 81-102. The Soviet controversy may be compared with the debate among non-Soviet Marxists on the same problem in the history of Western Europe. See Paul Sweezy, ed., *The Transition from Feudalism to Capitalism* (London, 1959).

<sup>4</sup> Students of Russian historical writing are familiar with M. N. Pokrovskii's ill-starred attempt to prove the dominance of commercial capitalism in seventeenth-century Russia. After he and his work were denounced in the early 1930s something of a vacuum existed on the matter of the genesis of capitalism in Russia. The question figured prominently again in the protracted discussions of periodization that occurred in the years following the Second World War. The vacuum was soon filled by the conception shortly to be described. See Konstantin Shteppa, *Russian Historians and the Soviet State* (New Brunswick, 1962), 114-15, 246-75; and Leo Yaresh, "The Problem of Periodization," in C. E. Black, ed., *Rewriting Russian History* (2d ed.; New York, 1962), 42-77.



formity was ever a fair characterization of Soviet work on this theme, it certainly is no longer, for the conference proceedings spotlight a lively controversy of absorbing interest.

The conference centered on a committee report, which sets out the broad range of questions in dispute, including such basic matters as the time the process began and the manner in which it unfolded. On the first point, a few Soviet historians—for example, D. P. Makovskii and Academician S. G. Strumilin—had opted for the sixteenth century, and a large majority for the seventeenth.<sup>5</sup> The committee report rejects both these views and fixes the beginning of the genesis of capitalism in Russia in the 1760s. To the proponents of the seventeenth century the transitional period in Russia, though exhibiting some variations, appeared fundamentally similar to that in Western Europe. In the committee report similarities of a very general kind are acknowledged, but they are definitely overshadowed by profound differences that impart to the Russian historical process its distinctive character. The report canvasses not only substantive but theoretical and methodological issues as well, and indicts modes of investigation and interpretation that brought to predominance what it takes to be a lamentably erroneous conception of a highly significant slice of Russian history. The treatment of these matters makes plain that many of our misgivings about much of Soviet historiography are shared by numerous members of the Soviet historical guild.<sup>6</sup>

It will be readily appreciated that the conference report could not have been a bolt out of the blue. Over a period of time a paradigm for the transition from feudalism to capitalism in Russia had taken shape and served as the framework for research.<sup>7</sup> Beginning in the mid-1950s, however, the findings and reflections of a number of investigators brought one facet of the paradigm after another into question. The doubts voiced, sometimes implicitly and sometimes explicitly, by such historians as N. M. Druzhinin, N. L. Rubinshtein, V. K. Iatsunskii, S. D. Skazkin, and N. I. Pavlenko, apparently went unheeded.<sup>8</sup> But as additional contrary evidence

<sup>5</sup> For a generally positive review of D. P. Makovskii's book (see note 20 below), which included an approving introduction by S. G. Strumilin, see Richard Hellie "The Foundations of Russian Capitalism," *Slavic Review*, 26 (1967): 148–54.

<sup>6</sup> For example, the first pages of the committee report make many of the very same critical points on methodology and interpretation as I did in my review essay of N. M. Druzhinin *et al.*, eds., *Goroda feodal'noi Rossii* (The Towns of Feudal Russia) (Moscow, 1966); see Baron, "The Town in 'Feudal' Russia," *Slavic Review*, 28 (1969): 116–22.

<sup>7</sup> I use here T. H. Kuhn's terminology, for what has occurred in Soviet historiography on the transition from feudalism to capitalism is reminiscent of, though not identical with, the pattern Kuhn described in his book *The Structure of Scientific Revolutions* (2d ed.; Chicago, 1970).

<sup>8</sup> A very important publication implicitly critical of the paradigm was Druzhinin's report to the International Congress of Historians in Rome in 1955, entitled *Genezis kapitalizma v Rossii* (The Genesis of Capitalism in Russia) (Moscow, 1955). V. K. Iatsunskii, a more combative historian, polemicized against the prevailing view in his review of N. V. Ustiugov's highly touted work on the salt industry at Solikamsk (see note 12 below) in *Istoriia SSSR*, no. 1 (1958): 193–96; and in his article, "Osnovnye etapy genezisa kapitalizma v Rossii" (Fundamental Stages of the Genesis of Capitalism in Russia), *ibid.*, no. 5 (1958): 59–91. In a symposium in 1959 Iatsunskii, Academician S. D. Skazkin, and A. L. Shapiro constituted a panel that broadened the attack. Their papers are published in *Ezhegodnik po agrarnoi istorii vostochnoi Evropy 1959 g.* (An-

accumulated, the doubters evidently coalesced into a group prepared to do battle for their convictions. The conference in 1965 was a confrontation between opposing schools, and the initiative lay with the insurgents. The committee report they produced summarized and synthesized the evidence developed over a period of at least a decade for an alternative view of the Russian historical process. A trenchant critique of the paradigm, it not surprisingly called forth counterattacks and strenuous objections to what was dubbed its "historiographical nihilism." Although a few points were scored against them, the committee and its adherents nevertheless plainly carried the day, and subsequent developments appear to confirm their victory.<sup>9</sup> Is it fanciful to see in all this something of a revolution in Soviet historiography?

In the years just after the Second World War Soviet historians found in a statement Lenin made in 1894 a principal guideline for their research.

Only the new period of Russian history (approximately from the seventeenth century) is characterized by the actual amalgamation of all . . . the regions, lands, and principalities into one whole. This amalgamation . . . was brought about by the increasing exchange among regions, the gradually growing circulation of commodities, and the concentration of small local markets into a single, all-Russian market. Since the leaders and masters of the process were merchant capitalists, the creation of these national ties were nothing else than the creation of bourgeois ties.<sup>10</sup>

The extraordinary significance attached to the statement may be gauged by the words of two principals in the controversy, A. A. Preobrazhenskii and Iu. Tikhonov, who have claimed that it "provides the key to the

nual on the Agrarian History of Eastern Europe: 1959) (Moscow, 1961), 21-68. N. L. Rubinshtein was the first Soviet historian, to my knowledge, to call into question the interpretation put upon one of Lenin's statements, which, as will shortly appear, plays a central role in the controversy. See his "Territorial'noe razdelenie truda i razvitie vsersossiiskogo rynka" (The Territorial Division of Labor and the Development of the All-Russian Market), in V. V. Al'tman, ed., *Iz istorii rabochego klassa i revoliutsionnogo dvizheniia: Sbornik statei pamiati Akademika Anny Mikhailovny Pankratovoi* (On the History of the Working Class and the Revolutionary Movement: A Collection of Articles in Memory of Academician Anna Mikhailovna Pankratova) (Moscow, 1958), 87-100. Pavlenko's studies of the metallurgical industry in the eighteenth century turned up irrefutable evidence of the very special character of Russia's socioeconomic development; see especially his *Istoriia metallurgii v Rossii XVIII veka* (History of Metallurgy in Eighteenth-Century Russia) (Moscow, 1962).

<sup>9</sup> The recommendations that grew out of the conference materials and are a part of the printed record clearly aimed to strike a compromise, but this in no way conceals the fact that the insurgents routed their adversaries. This conclusion is confirmed by examination of the follow-up statements of the contending groups (see the two articles cited in note 3 above). The turgidity of the first and the brilliance of the second correspond roughly to the merit of the arguments advanced by each side. E. I. Zaozerskaia's recent study of Russian industry in early modern times, *U istokov krupnogo proizvodstva v russkoi promyshlennosti XVI-XVII vekov* (The Sources of Large-Scale Production in Russian Industry of the Sixteenth through Seventeenth Centuries) (Moscow, 1970), adds further support to the case advanced in the 1965 committee report. The proponents of the disenthroned outlook have not surrendered, but it seems true that something like the views advanced by the committee report now have the support of the greater number of Soviet historians concerned with this problem.

<sup>10</sup> The statement occurred in a polemic against the Populist writer N. K. Mikhailovskii in the pamphlet "Who Are the 'Friends of the People' and How They Fight against the Social Democrats?" See V. I. Lenin, *Collected Works* (Moscow, 1963-68), 1: 154-55.

understanding of a whole epoch in the history of our motherland.”<sup>11</sup> Its critical importance evidently rested on three counts: first, the phrase “the new period” demarcated a major division of Russia’s history; second, the lines following that phrase sketched the content of the “new period,” perceived as nothing other than the genesis of capitalism; and third, the beginning of the process was fixed in the seventeenth century.

If Lenin supplied the basis of periodization, Soviet investigators derived from Marx the stages and criteria, the categories of thought, and the modes of analysis wherewith to trace the evolution from feudalism to capitalism. Armed with these concepts, students of Russia’s socioeconomic history in the centuries preceding the abolition of serfdom have worked through vast masses of archival material in search of evidence to underpin them. The countless articles, hundreds of dissertations, and scores of book-length monographs that resulted invariably recited Lenin’s statement of 1894 and professed to have corroborated it. For the seventeenth century, evidence was adduced for the separation of agriculture from nonagricultural production in a developing social division of labor; the gradual replacement of a natural, self-sufficient economy by a national market based on commodity production, territorial division of labor, and greatly increased significance of towns and merchants; the rise of large-scale industrial enterprises (manufactories) employing hired labor; and social differentiation in town and countryside, which bespoke the primary accumulation of capital in the hands of a nascent bourgeoisie and the divorce of significant numbers of producers from the means of production.<sup>12</sup>

<sup>11</sup> See A. A. Preobrazhenskii and Iu. A. Tikhonov, “Itogi izucheniia nachal’nogo etapa skladyvaniia vs Rossiiskogo rynka (XVII v.)” (The Results of Study of the First Step in the Formation of the All-Russian Market [Seventeenth Century]), *Voprosy Istorii*, no. 4 (1961): 80. This article summarizes the evidence accumulated up to that time in support of what I have called the paradigm. Judgments similar to the one quoted are frequently encountered in the literature.

<sup>12</sup> Among representative works one finds such instructive titles as: *Voprosy genezisa kapitalizma v Rossii* (Problems of the Genesis of Capitalism in Russia) (Leningrad, 1960); L. Beskrovny *et al.*, eds., *K voprosu o pervonachal’nom nakoplenii v Rossii (XVII–XVIII vv.)* (On the Problem of Primary Accumulation in Russia [Seventeenth through Eighteenth Centuries]) (Moscow, 1958); A. Ts. Merzon and Iu. A. Tikhonov, *Rynok Ustiuga Velikogo v period skladyvaniia vs Rossiiskogo rynka (XVII vek)* (The Market at Ustiug the Great in the Period of the All-Russian Market in Formation [Seventeenth Century]) (Moscow, 1960); N. V. Ustiugov, *Solevarennnaia promyshlennost’ Soli Kamskoi v XVII veke* (The Saltworks Industry in Solikamsk in the Seventeenth Century) (Moscow, 1957); N. A. Baklanova, *Torgovo-promyshlennnaia deiatel’nost’ Kalmykovykh vo vtoroi polovine XVII v. K istorii formirovaniia russkoi burzhuzii* (The Commercial-Industrial Activity of the Kalmykovs in the Last Half of the Seventeenth Century: On the History of the Formation of the Russian Bourgeoisie) (Moscow, 1959); A. M. Pankratova, *Formirovanie proletariata v Rossii (XVII–XVIII vv.)* (The Formation of the Proletariat in Russia [Seventeenth through Eighteenth Centuries]) (Moscow, 1963); and Druzhinin, *Goroda feodal’noi Rossii*. These titles suggest the great efforts invested in demonstrating that the seventeenth century was indeed the seedbed of Russian capitalism. All the works named, however, cannot fairly be assigned to one neat category. If some were consciously designed to illustrate particular formulas, others exhibit a decided independence of spirit. For example, in spite of the title given her book, Pankratova forthrightly declares her disagreement with those who discern capitalistic relations in seventeenth-century Russia: “There can be no talk of capitalism or a proletariat in the Marxian sense in the seventeenth or even the eighteenth centuries, except for the last two or three decades of the latter.” *Formirovanie proletariata*, 9. The views she develops on Russian towns (pp. 82–85) are also markedly contrary to the ebullient portrayal of seventeenth- and eighteenth-century towns given by most contributors to Druzhinin, *Goroda feodal’noi Rossii*.

The prevailing interpretation of the seventeenth century deeply colored the treatment of what followed. If the genesis of capitalism was already well begun, then it was only logical, indeed "lawful" (*zakonomernyi*), that it should continue to develop in extent and depth.<sup>13</sup> The dominant Soviet historiography discerned the more or less smooth build-up of the indexes cited earlier, the passage of quantitative increases into qualitative changes, and the maturing of the crisis of the feudal regime. Contradictions generated between modes of production increasingly capitalistic in nature and relations of production colored by feudalism found expression in class struggle. In the first half of the nineteenth century the contradictions became ever more acute, created a revolutionary situation in the later 1850s, and ultimately made inescapable the abolition of serfdom. Works rooted in the paradigm not infrequently included qualifications that appear to cast doubt on its validity, but in the final analysis they were generally minimized or completely left out of account.<sup>14</sup> As a consequence, even though it brought masses of facts into play, the dominant rendering of this period of Russian history tended to be both highly schematic and misleading.

The committee found especially telling weapons for its assault upon the paradigm in the writings of Druzhinin and A. G. Man'kov. Man'kov, author of virtually the only postwar, book-length Soviet work on the seventeenth century that ran against the current, had called attention to "the peculiar situation that [had come] to prevail in [Soviet] historiography." Most authors, he observed, are concerned with "the search for 'new phenomena' in the economy of seventeenth-century Russia, by which they understand the rudiments of capitalist relations in the spheres of both commodity production and exchange. Meanwhile [they] take into account only to a very feeble extent the dominant feudal-serf relations, which continued to grow in strength." Such investigators paid only lip service to what was dominant while concentrating their attention on tendencies of a relatively minor order. The greater part of Soviet research on the entire transitional

<sup>13</sup> The lawfulness (*zakonomernost'*) of the historical process has of course been a central concept of Soviet historiography. I agree with the observations of Arthur P. Mendel ("Current Soviet Theory of History," 57) and Hans Rogger ("Politics, Ideology, and History in the USSR," 266-68) on the ambiguity of the concept in recent Soviet historical writing.

<sup>14</sup> To illustrate, Tikhonov wrote of the Ustiuga area: "The deepening social division of labor was conditioned by the growth of commercialization of agriculture in the seventeenth century." After this unequivocal assertion we read: "It was not possible to establish quantitatively the relationship between the value of agricultural production and the share marketed." A bit further along he adds: "However, there is no need to exaggerate the extent of development of commodity production in the feudal village. By no means all the peasant economies were closely linked to the market. Even part of the richest of the peasants [and] some townsmen were still only going over to regular sale of their products. Many middlemen did not carry on market operations systematically." Merzon and Tikhonov, *Rynok Ustiuga Velikogo*, 657-58. Despite the qualifications, the initial quotation is taken to define the situation. At the conference Iatsunskii destroyed its last shred of credibility. He had calculated the ratio of marketed to harvested grain for the individual peasant household of the region and found it to average two per cent. *Perekhod*, 267.

period, Man'kov plainly implied, represented Russian historical reality as in a distorting mirror.<sup>15</sup>

The committee underscored the need to attend not only to the old as well as the new but also to the relative weight of the one as against the other and to the effects each produced upon the other. It deplored the narrowly economic focus of much of Soviet research and the failure to give due consideration to a whole range of other forces and factors—such as the merchants and gentry, the reverse actions of the political superstructure on the economic base, and the role of foreign relations and influences—whose neglect made it impossible to perceive the Russian historical process rightly, and in its wholeness. The committee endorsed the use of the comparative-historical method as an effective way to achieve understanding of “the general and the unique in their unity . . . to reveal the *different* forms of the historical process.”<sup>16</sup>

Following some such principles Druzhinin had ten years earlier produced an interpretation of the genesis of capitalism in Russia that, with some qualification and considerable supplementation, the committee embraced. Druzhinin had emphasized not the similarity but the contrast between the Russian and West European transition from feudalism to capitalism. Capitalistic elements began to appear in Russia not after the liquidation of serfdom as in the West but while feudal-serf relations were still developing vigorously. Russia did not participate in the overseas expansion that spurred the commercial revolution in the West. Instead it long remained essentially a country of natural economy with a weakly developed commercial-industrial sector and an inconsequential bourgeoisie. In Russia primary accumulation of capital and the formation of a free labor force proceeded very slowly, and, until the second half of the eighteenth century, much of the industrial base stemmed from state initiative and relied heavily on compulsory labor. Capitalistic development in Russia proceeded simultaneously with the further development of a powerfully entrenched feudalism, which displayed a notable ability to adapt to changing needs and circumstances. Its success in assimilating elements of the new was nowhere better expressed than in the conduct of the nascent

<sup>15</sup> A. G. Man'kov, *Razvitie krepostnogo prava v Rossii vo vtoroi polovine XVII veka* (The Development of Serfdom in Russia in the Second Half of the Seventeenth Century) (Moscow, 1962), 5–6. Man'kov's book focused on the extension and strengthening of the so-called feudal-serf order in the last half of the seventeenth century. Among other things he emphasized that the town populations were reduced to a condition analogous to that of peasant-serfs (see ch. 4).

<sup>16</sup> *Perekhod*, 6; italics mine. The report located in the era of the “cult of personality” a drive to assimilate the national histories of all countries [read: Russia and the West European lands] to a single [read: Western] pattern, which had as its complement the discontinuation of comparative-historical studies. Soviet historians, the committee maintained, unfortunately tended to see Russia's transition from feudalism to capitalism as similar to the “classical” English model (p. 6). This criticism is directed against a too mechanical transposition of Marx's formulas, derived from his extended study of English history, to a radically different context.



bourgeoisie, which essentially accommodated itself to, rather than struggled against, the existing order.<sup>17</sup>

Druzhinin included the seventeenth century in the transitional period, although the general contours of his interpretation would seem to allow for only the most feeble beginnings there. Others directly contradicted the claims for the seventeenth century made above all by N. V. Ustiugov and his followers. In a much-cited article published in 1950 Ustiugov had discovered a significant movement in the seventeenth century from handicraft production for use or local order to commodity production—i.e., a strong drive toward market relations. He wrote a painstaking study of the salt industry at Solikamsk, the greatest center of seventeenth-century Russia's most important industry, and found distinctly capitalistic tendencies there.<sup>18</sup> Under his influence, many other researchers undertook to demonstrate the existence of the same commercial and industrial phenomena in different localities or lines of production. Ustiugov also edited several important volumes designed to flesh out the conception he championed, which is well rendered by the title of one of them: *The Russian State in the Seventeenth Century: New Phenomena in Socioeconomic, Political, and Cultural Life*.<sup>19</sup>

Attacks upon the claims for the seventeenth century, both earlier and in the committee report, were two-pronged. On one hand, they denied the interpretation put upon the evidence adduced; on the other, they contested the prevailing interpretation of Lenin's 1894 statement, which the evidence was supposed to corroborate. The search for the new, the critics asserted, had misled overzealous investigators to antedate the appearance of capitalistic elements or to exaggerate the significance of such

<sup>17</sup> Druzhinin, *Genezis kapitalizma v Rossii*, 7–12, 18, 34, 37.

<sup>18</sup> N. V. Ustiugov, "Remeslo i melkoe tovarnoe proizvodstvo v Russkom gosudarstve XVII v." (Handicrafts and Small-Scale Commodity Production in the Seventeenth-Century Russian State), *Istoricheskie Zapiski*, 34 (1950): 166–97; Ustiugov, *Solevarenaia promyshlennost' Soli Kamskoi v XVII veke*.

<sup>19</sup> Ustiugov, ed., *Russkoe gosudarstvo v XVII veke. Novye iavleniia v sotsial'no-ekonomicheskoi, politicheskoi, i kul'turnoi zhizni* (The Russian State in the Seventeenth Century: New Phenomena in Socioeconomic, Political, and Cultural Life) (Moscow, 1961). Ustiugov also edited the volume by Merzon and Tikhonov (see note 12 above), which purportedly confirmed beyond a doubt the case for capitalistic market relations in the seventeenth century, even as Ustiugov's study of the salt industry was to have demonstrated the capitalistic character of production. One of the two editors of *Ocherki istorii SSSR. Period feodalizma: XVII v.* (Essays in the History of the USSR. The Feudal Period: The Seventeenth Century) (Moscow, 1955), the volume on the seventeenth century in the most important multivolume general work of the last decades, Ustiugov also contributed more heavily to the volume's sections on the economy than any other writer. He plainly exerted a wider influence, but his two most conspicuous disciples were and are Preobrazhenskii and Tikhonov. In 1961 the two produced an article (see note 11 above) summarizing the evidence thus far discovered in favor of the seventeenth century as the period when capitalism began to develop in Russia. Preobrazhenskii contributed an appreciation of his master to the posthumous *Festschrift* to Ustiugov, fittingly entitled *The Towns of Feudal Russia*. Oddly enough Ustiugov was scarcely mentioned, much less directly attacked, in the 1965 committee report, although it is plain that he was one, if not the, major architect of the conception under fire. Perhaps this is to be explained by reluctance to assail a recently deceased (1963) and personally respected and well-liked comrade. Contributors to the discussion frequently cited Ustiugov's interpretations, however—sometimes approvingly, sometimes skeptically.



as actually did arise.<sup>20</sup> The larger industrial enterprises of the seventeenth century were few in number and frequently financed by foreign capital. In any case they could not be characterized as capitalistic manufactories because they involved no significant change in technology or in the organization of production. Market relations in the seventeenth century were no more capitalistic. To be sure, commodity production increased, but production for sale occurred to a limited extent everywhere while feudalism prevailed. The same was true of economic inequality among the peasants and the employment of hired labor, evidence of which was erroneously cited as proof of the advent of capitalistic features in the seventeenth century. Least of all was anything resembling early capitalist relationships to be found in agriculture, the most important branch of the economy.

For the critics, the Solikamsk salt industry and the trade activity of the White Sea region (*Pomor'e*), so important to their opponents' case, actually possessed only a local and transitory rather than a national and transforming significance. Special local circumstances accounted for the development of the salt industry and trade activity, and in the eighteenth century the one and the other were eclipsed.<sup>21</sup> Similarly almost none of the great merchant families that arose in the seventeenth century survived into the eighteenth.<sup>22</sup> If some rudiments of capitalistic relations appeared in

<sup>20</sup> The predating of the advent of capitalistic relations in Russia was a principal object of criticism in the papers by Skazkin, Iatsunskii, and Shapiro delivered to the conference on East European agrarian history in 1959. The extreme to which this tendency might be pushed is illustrated in the work of Makovskii, who proposed in all seriousness that in the sixteenth century "the trade turnover in the markets of [no] country in Europe could have been greater than Russia's." One of the participants in the 1965 conference, M. T. Beliavskii, remarked ironically that someone might take it into his head to fix the beginning of Russian capitalism's genesis in the Bronze Age. See *Ezhegodnik po agrarnoi istorii vostochnoi Evropy 1959 g.*, 21, 26-28, 32-33, 48-49, 52-54; D. P. Makovskii, *Razvitiie tovarno-denezhnykh otnoshenii v sel'skom khoziaistve russkogo gosudarstva v XVI veke* (The Development of Commodity-Money Relations in the Agriculture of the Seventeenth-Century Russian State) (Smolensk, 1963), 77; *Perekhod*, 308.

<sup>21</sup> A good many of these arguments had already been adduced in the critical writings referred to in note 8 above. The committee report stresses the multiformity of the historical process in Russia, the large regional variations, which made it hazardous to draw generalizations from the developments disclosed in one area. *Perekhod*, 5, 112. Zaozerskaia's recent book further spikes the claims made for the salt industry and shows that other industries in seventeenth-century Russia were more backward. Not only did technology in the salt industry remain stagnant for centuries, but in the seventeenth century the industry's entrepreneurs persistently strove to convert the workers they hired into a compulsory labor force. See Zaozerskaia, *U istokov krupnogo proizvodstva v russkoi promyshlennosti XVI-XVII vekov*, 64-66, 98, 446-50; she directly contradicts Ustiugov's claims for the salt industry on pp. 49, 151, 175-78, 185.

<sup>22</sup> This very important point emerged initially in S. V. Bakhrushin's work and was confirmed by the researches of K. V. Bazilevich, N. A. Baklanova, and V. A. Aleksandrov. See Bakhrushin's articles, "Promyshlennye predpriiia russkikh torgovykh liudei v XVII v." (The Industrial Enterprises of the Seventeenth-Century Merchants) and "Torgi gostia Nikitina v Sibiri i Kitae" (The Commercial Enterprises of Gost' Nikitin in Siberia and China), in his *Nauchnye trudy* (Moscow, 1952-59): vols. 2, 3: pt. 1; K. V. Bazilevich, "Krupnoe torgovoe predpriatie (Bosovykh) v Moskovskom gosudarstve v pervoi polovine XVII veka" (A Great Trading Enterprise [the Bosovs'] in the Moscow State in the First Half of the Seventeenth Century), *Izvestiia Akademii Nauk SSSR, Otdelenie obshchestvennykh nauk*, ser. 7, no. 4 (1932): 783-811; and V. A. Aleksandrov, "Sibirskie torgovye liudi Ushakovy" (The Siberian Merchants—the Ushakovs), in Ustiugov, *Russkoe gosudarstvo v XVII veke*, 131-50. Baklanova's study of the Kalmykovs (see note 12 above) reveals that they suffered a like fate. No Soviet writer has attempted to deter-

seventeenth-century Russia, the committee report concluded, they were sporadic in nature, unstable, and either could not survive in or were deformed by the feudal-serf milieu, whose evolution was then the decisive factor in the country's development.

In 1958 and 1959 Rubinshtein, Academician Skazkin, and others had taken issue with the prevalent interpretation of Lenin's 1894 statement. Skazkin gently hinted at the imprudence of treating as holy writ a brief remark Lenin had made in the heat of a polemic against the populist N. K. Mikhailovskii. Less persuasively, he suggested that when Lenin referred to "the creation of bourgeois ties" he had in mind something quite different from what his interpreters supposed. Neither he nor anyone else pointed out that in 1894 Lenin was a mere twenty-four years of age, and no matter how great his genius he could have possessed little knowledge of seventeenth-century socioeconomic history, if only because the study of the subject had scarcely begun.<sup>23</sup> Rubinshtein and the authors of the report took a different but effective line: they used the authority of the more mature Lenin, the author of *The Development of Capitalism in Russia* and subsequent works, to refute the interpretation widely imputed to Lenin's 1894 statement. Lenin had been intensely aware of the powerful influence of feudal-serf relations on the course of Russian history, and he knew the great difficulties capitalism had in making its way even as late as the first half of the nineteenth century. His more fully elaborated views were hardly consistent with the exaggerated claims Soviet historians made for the seventeenth century, and they also stressed the very special qualities that sharply distinguished Russia's evolution from that of Western Europe.<sup>24</sup>

Among the peculiarities of the Russian historical process the committee stressed uneven development and what others have called combined development. Unevenness refers both to Russia's lag behind the West and the differential rates and modes of socioeconomic change in different parts of the vast Russian realm; combined development to the blending of old forms with new in distinctive arrangements, particularly as a result of borrowing that was calculated to overcome the lag. Thus in the first half of the eighteenth century military exigency impelled the state to import Western technology as a means of promoting large-scale industrial development—but that development was essentially noncapitalistic in type. Large

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mine systematically the survival rate of the big commercial operators. An investigation I have made, which is to be published in *California Slavic Studies*, reveals that in the seventeenth century only one great merchant family in four managed to perpetuate its status longer than one generation and almost none longer than two.

<sup>23</sup> The committee report and Pavlenko's follow-up article in 1966 both embraced Skazkin's view of Lenin's statement, but A. M. Sakharov, one of the conferees, justifiably cast doubt on this tortured interpretation. Pavlenko remarked in his article that little was known of seventeenth-century socioeconomic history when Lenin made his pronouncement. See *Perekhod*, 25, 190; Pavlenko, "Spornye voprosy genezisa kapitalizma v Rossii," 82, 88.

<sup>24</sup> Rubinshtein, "Territorial'noe razdelenie truda i razvitie vserossiiskogo rynka," 89-90; *Perekhod*, 38-40, 52, *passim*.

enterprises most untypical of a feudal economy arose, but they were bent to the norms of the feudal-serf order. The working force consisted in the main of compulsory-serf rather than free-hired labor. A goodly share of the enterprises came under the control of the landowning gentry, and merchant-industrialists, the nearest thing Russia had to a bourgeoisie, wanted nothing more than the right to exploit serf labor and to attain gentry status. The feudal-serf order demonstrated its resiliency, its ability to promote the development of productive forces without changing the basic social arrangements.<sup>25</sup>

The committee report presents persuasive evidence for a beginning of genuinely capitalistic relationships in a sector of Russian industry in the 1760s. The characterization of the next hundred years, which culminated in the emancipation of the serfs, however, is equivocal and less compelling. To its credit the committee scotches some dubious claims advanced by certain Soviet historians: that the peasant revolt led by Pugachev "objectively" aimed at the establishment of a bourgeois order and that Russia went through an industrial revolution in the decades preceding the abolition of serfdom.<sup>26</sup> While rejecting viewpoints that plainly derive from ideology rather than evidence, the committee report itself is not entirely free from the same fault. It characterizes the century in question as the period in which the march of capitalism became an irreversible process, when its advance progressively eroded and undermined the feudal-serf order until that system entered upon its final crisis. Evidence for such an interpretation is not wanting, but this view ultimately fails, for the "contradictions" were of course resolved through a reform from above rather than a revolution from below. Though not articulated into a whole, the elements of an alternative—and more convincing—interpretation are to be found in the report. More than once it suggests that even in the first half of the nine-

<sup>25</sup> The image of eighteenth-century socioeconomic development depicted in the committee report derives in considerable part from Pavlenko's studies. G. V. Plekhanov applied conceptions approximating to the theory of uneven and combined development to Russia to a limited extent, but it was particularly Trotsky who in 1906 developed this approach in an arresting manner. Soviet historians consider Lenin as the progenitor of the theory, but irrespective of its source the impulse of Soviet historians to utilize this illuminating approach is not the least interesting recent development in Soviet historiography. In the West, Alexander Gerschenkron has most fruitfully analyzed Russia's economic development in some such terms. The paradoxical view advanced at the 1965 conference that Peter the Great's consolidation of "feudal-serf" relations gave momentum to Russian economic development was anticipated by Gerschenkron. See S. H. Baron, *Plekhanov, the Father of Russian Marxism* (Stanford, 1963), 113-14, 355-57; Isaac Deutscher, *The Prophet Armed: Trotsky, 1879-1921* (New York, 1954), 148-54; and Alexander Gerschenkron, *Economic Backwardness in Historical Perspective* (New York, 1962); for the point on Peter the Great, see pp. 18, 135, 153-54.

<sup>26</sup> The committee report also argued that the Cossacks were the key element in the "peasant wars" (*Perekhod*, 32-33)—a position hotly disputed by a number of conferees. Earlier writers who had espoused the position the committee now affirmed had been sharply criticized in *Ocherki istorii SSSR. Period feodalizma: XVII v.*, 9-13. The argument advanced for a pre-emancipation industrial revolution, notably by Academician Strumilin, was, in the opinion of the committee, informed by a tendency in the 1940s "to exaggerate the technical-economic attainments of tsarist Russia." *Perekhod*, 72.

teenth century the feudal-serf order still gave some indications of vitality; it still had not exhausted its potentialities. The "crisis" might have been protracted indefinitely, we may infer, and all the more so because, as the committee acknowledges (following Lenin), the revolutionary forces in the mid-nineteenth century were insignificant.<sup>27</sup> Seen from this angle Russia's defeat in the Crimean War rather than the specter of revolution becomes the critical factor in the decision to abolish serfdom.

The comments on the report, though uneven in quality, offer fascinating insights into the workings of the Soviet historical guild. The most penetrating and impressive remarks came from the report's supporters (notably Pavlenko, A. L. Shapiro, L. V. Danilova, M. T. Beliavskii, M. Ia. Gefter, and Iatsunskii), from A. M. Sakharov, who shared the committee's critical spirit but disagreed with some of its findings, and from Iu. Iu. Kakhk, who displays notable analytical gifts. The upholders of the paradigm came off rather badly, and none worse than Preobrazhenskii. After Pavlenko and Sakharov, among others, had castigated the substitution of "citationism" for profound study of theory and method, Preobrazhenskii proceeded to defend the construction under attack with a long string of quotations from Marx-Engels and Lenin. Insisting once again that Lenin's characterization of the "new period" remained the correct basis for grasping the essence of seventeenth-century Russia, Preobrazhenskii strikingly confirmed the charges made against his group: in listing the "elements of the new" in the seventeenth century, he neglected to mention what was surely the most important of all—the definitive establishment of serfdom in the law code of 1649. Shaken by the attack, most of the defenders seemed to fall back on a more modest position: weak though the rudiments of capitalism earlier were, nevertheless they existed, accumulated, and laid the basis for the breakthrough of the 1760s, so that the preceding century or so should be included in the transitional period.<sup>28</sup>

<sup>27</sup> The net effect of the report here is to undercut the dubious claims of Academician M. V. Nechkina and others that a revolutionary situation existed in Russia in 1859–61. Contributors to the conference discussion spoke of the exaggerated emphasis given peasant disturbances in explanations of the decision to abolish serfdom, and there was evident a sense of the incongruity of speaking of a revolutionary situation in the absence of a revolutionary class. *Perekhod*, 200, 271–72, 316–17. For a recent appraisal by an American scholar of Nechkina's position, see Charles Adler, Jr., "The 'Revolutionary Situation 1859–1861': The Uses of an Historical Conception," *Canadian Slavic Studies*, 3 (1969): 383–99. The existence in the report of elements of two different explanations of the abolition of serfdom reflects some difference of opinion among members of the committee. Among these differences, according to Pavlenko, was the representation of the "crisis of the feudal-serf system"; another was "the role of the state and the influence of its policy on the socioeconomic processes of the transitional period." *Perekhod*, 109. Ryndziunskii and others also referred to differences within the committee (p. 204). In addition disagreements existed among those who generally supported the paradigm. For example, the views of Troitskii and M. Ia. Volkov on the character of the state in the seventeenth and eighteenth centuries were sharply at variance (pp. 139–40, 151–55).

<sup>28</sup> For Preobrazhenskii's remarks, see *Perekhod*, 213–22. The critics of "citationism" pointed out that great questions could not be settled by a quotation and that in any case one could find support in the "classics" for conflicting points of view (pp. 223–24, 113, 188–89). For the stance the upholders of the paradigm assumed after the conference, see Bulygin, Indova, Preobrazhenskii, Tikhonov, and Troitskii, "Nachal'nyi etap genezisa kapitalizma v Rossii." The group inadvertently made a most damning admission when it argued that Lenin had based his statement on

Like the report, the discussion was remarkable for its demonstration of self-criticism and a mutual criticism expressed without fear or favor. Reference was made to the deplorable effects of the "cult of personality" on historical work, among them efforts to prove Russian priority in all things—"including even the transformation of free people into serfs." E. M. Zhukov identified the often dogmatic rather than dialectical use made of theoretical propositions as the source of "the increased skepticism of our academic youth." Shapiro reminded his colleagues of the historian's obligation "to reconstruct an objective picture [rather than] to write what is congenial to the spirit." Historians of lesser rank rebuked several academicians—the most exalted members of the guild: Strumilin, whose imperfect understanding of the "classics" and faulty reasoning had led him to posit seriously mistaken views on major questions; M. V. Nechkina, whose brief for a revolutionary situation in the late 1850s was quietly shelved and whose objections to the report were termed "mechanistic"; and L. V. Cherepnin, for, among other things, having unjustly charged the committee with expounding Man'kov's views incorrectly.<sup>29</sup>

Certain of the substantive and methodological views expressed exhibited a refreshing openness, a readiness to entertain ideas once considered taboo. The committee acknowledged the tentativeness of some of the positions it took and pointed to many questions that demanded further investigation. Both its members and various contributors to the discussion offered helpful suggestions for attaining greater precision (quantitative studies) and called for the careful formulation of criteria for demarcating the stages of development in the transition from feudalism to capitalism. Sakharov asked his colleagues to compare Russia's historical evolution not just with that of Western Europe but with the countries of the East as well. Some of those who attacked the report implied that its authors regarded the feudal-serf order in the seventeenth and eighteenth centuries not only as progressing but—*horribile dictu!*—also as progressive. Sakharov, P. G. Ryndziunskii, and others openly declared that the feudal-serf order was indeed progressive and, moreover, essential to the nation's very existence at a certain stage of its development, and that it should be judged historically and not by twentieth-century standards. Two conferees challenged the prevailing view, evidently maintained with some differences in detail by both sides, of the character of the Russian state in the seventeenth and eighteenth centuries. In M. Ia. Volkov's opinion, it should not be likened to the European absolute monarchies for it was closer to what Marx had called "the Asiatic mode of rule."<sup>30</sup>

the seventeenth century not on Russian historical works but on pronouncements of Marx and Engels (p. 82). It follows that a statement with no historical basis whatever was uncritically accepted as the authoritative guide to the understanding of a whole epoch by almost an entire generation of Soviet historians.

<sup>29</sup> *Perekhod*, 192, 106, 185, 10, 73–74, 13, 333, 304, 381–85.

<sup>30</sup> For the remarks of Sakharov, Volkov, and also V. I. Koretskii on Russia and the Oriental countries, see *ibid.*, 192, 149–50, 399. These comments recall Plekhanov's interesting conception of



After examining the sketch of Russian history set out by the committee, the Western student is apt to exclaim: "But this interpretation is not so different from our own!" Quite so, and this convergence deserves emphasis. Still, one may be somewhat perplexed by the similarities, for the critical group of Soviet historians shows no inclination whatever to abandon Marxism-Leninism as the basis of historical research and understanding. We note, however, that the committee report and some of the commentators draw a distinction between a mechanistic, "logical" (aprioristic), citation-ridden, and therefore illegitimate employment of Marxism-Leninism and its more flexible, sensitive, dialectical use. I would translate this language into a distinction between poor and sound historical method. What may appear as an ingenuous and question-begging judgment may be tested in two ways. First, the specific methodological dicta commended by the critics command assent; among other things, they require balance in research and interpretation as against one-sidedness, the priority of evidence over formulas, a use of the comparative-historical method calculated to disclose the relationship between the general and the unique. Second, if the proof of the pudding is in the eating, then the substantive findings in the committee report and the research on which it is based testify to the enormous superiority of the newer methodology to the old.<sup>31</sup> Soviet and Western students of at least some phases of Russian history are now dealing with what is recognizably the same "continent," and, contrary to the situation some years ago, dialogue now appears entirely possible.

Though all this is heartening, a few cautionary observations are in order. To all appearances the debate over the transition from feudalism to capitalism has been permitted to run its course, with the laurels going to the victor in a free competition of ideas. The constraints upon work in some of the more sensitive areas—such as the history of the Communist party of the Soviet Union and contemporary history—have diminished little if at all. We cannot know the extent to which the demythologizing tendencies evident in the controversy considered here may spill over into other historical realms or if the "crude administrative interference" of the 1930s, to which one of the conferees referred, may reappear in regions from which it had been withdrawn.<sup>32</sup>

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Russia as an Oriental or semi-Oriental despotism in the centuries under consideration. Such ideas were either angrily denounced or blithely ignored for some decades in Soviet historiography. See S. H. Baron "Plekhanov's Russia: The Impact of the West upon an 'Oriental' Society," *Journal of the History of Ideas*, 19 (1958): 388-404; and Karl Wittfogel, *Oriental Despotism* (New Haven, 1957), ch. 9.

<sup>31</sup> If my reasoning is correct, it should serve as an admonition that in dealing with Soviet historiography we not be put off by labels but are bound to look beyond to the substance of things.

<sup>32</sup> That both have occurred, however, is suggested by the recent case of A. Ia. Gurevich's *Problemy genezisa feodalizma v zapadnoi Evrope* (Problems of the Genesis of Feudalism in Western Europe). See David B. Miller's review in the *AHR*, 76 (1971): 756-57. That fresh winds are blowing in other areas of Soviet historiography has been made evident in review articles



Even on the transition from feudalism to capitalism in Russia of course differences between Soviet and Western scholars remain. To a certain extent they seem to me to be terminological rather than substantive, but they are also traceable to a lack of familiarity with other approaches—for example, the Weberian—to the problem of capitalistic development.<sup>33</sup> The distance has been reduced, in part, by greater willingness on the Soviet side to acknowledge the state's role in shaping Russia's socioeconomic history. But while the most perceptive Soviet historians have succeeded in avoiding the Scylla of "economic materialism," they have been so wary of the Charybdis of "statism" that they have still not accorded full value to state policy and action. One finds evidence enough of a better appreciation of the state now as obstructor, now as promoter of economic development, but this line of analysis seems never to be pursued systematically. If it were, Soviet historians might well find "the transition from feudalism to capitalism" a characterization incongruent with the "concrete-historical" facts of their country's past. Were such a perception to gain general acceptance, that would constitute a full-blown scientific revolution.

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that have appeared since the completion of the present essay. See John Meyendorff, "The Cambridge and Soviet Histories of the Byzantine Empire: Religious History and Theology," *Slavic Review*, 30 (1971): 619-23; and Alexander Gerschenkron, "Soviet Marxism and Absolutism," *ibid.*, 853-69.

<sup>33</sup> I have made a tentative application of certain of Weber's concepts to the Russian case in my article "The Weber Thesis and the Failure of Capitalist Development in 'Early Modern' Russia," *Jahrbücher für Geschichte Osteuropas*, 18 (1970): 321-36.

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## Reviews of Books

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### GENERAL

JULIÁN MARÍAS. *Generations: A Historical Method*. Translated by HAROLD C. RALEY. University: University of Alabama Press. 1970. Pp. xi, 220. \$7.50.

The book under consideration is one whose gist could easily be condensed into a ten-page essay. In fact the author obliges us by providing, at the end of his work, a résumé that sums up his views on the relationship between generational succession and the study of history. Throughout his monograph he focuses simultaneously on two tasks. One is to offer a theory of historical development based upon the formation and interaction of generations: units that he defines chronologically, sociologically, and, finally, with regard to their "epoch-making" capacities. He then attempts to prove that the vitalist philosophy of Ortega y Gasset, his own mentor, is the precondition for any proper understanding of historical generations. Supposedly, Ortega wrote conclusively on this subject in *El temo de nuestro tiempo* (1923).

An obvious question presents itself. Since Ortega had treated his topic in an exhaustive way, why should Marías have bothered to go over the same ground? Whatever the answer, one may be glad that he did. *Generations* is full of useful hints for conceptualizing the cycles of ideas and movements against the background of changing times. And apropos of demonstrating the value of Ortega's thought, Marías makes informative excursions into the writings of other Europeans concerned with the generation in history.

Although his treatment of Dilthey, Comte, Ranke, and other non-Spanish thinkers is sometimes grossly oversimplified, there is a sense of purpose that almost redeems the au-

thor's superficiality. Marías wishes to save his countrymen from the impulse of going, cap in hand, to the Germans and French to learn about historical methodology. Ortega, a Spaniard, through the perception of true historical contemporaneity, had made an original contribution to the field.

Mention should be made of Professor Raley's skill as a translator. His clear idiomatic language makes all of Marías's argumentative nuances accessible to American readers. Moreover, his introductory comments are admirably brief and unobtrusive.

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LEO BRAUDY. *Narrative Form in History and Fiction: Hume, Fielding, & Gibbon*. Princeton: Princeton University Press. 1970. Pp. viii, 318. \$10.00.

This remarkable monograph is about the artistic element in historical method, the structure of narrative. Braudy sees Hume, Fielding, and Gibbon as experimenting with narrative forms that not merely presented but controlled their historical materials. It was of course Fielding who through fiction could most explicitly impose an imaginative order on factual reality. But the prominence Braudy consequently accords to his novels—twice as much space as to Hume's *History* and three times as much as to Gibbon's *Decline and Fall*—should not mislead the reader into thinking that the study is concerned with "literature" rather than historiography.

Braudy finds a progressive development in each of his three writers. Emancipating themselves from traditional historiography, with its classical didacticism, they essayed different nar-

rative methods as their work went along. Hume, in his Stuart volumes, began with offering the traditional "character" explanations; he muted them in the Tudor volumes that followed, letting legal and institutional structures come to the fore; finally, in the volumes on the Middle Ages, he had recourse to interaction, contextual development, and the concept of the accretion of time. But Hume managed only to contain, not to control, his materials, presenting sometimes incompatible patterns, and not a consistent narrative voice. Fielding, concerned with private rather than public history, had more scope for redefining the historian's methods and tasks. In *Joseph Andrews* he rejected irrelevant factuality and distilled a factual sanction for authenticating history and fiction. In *Jonathan Wild* he attacked the use of providential and other transcendental explanations. His own program and practice are set out in *Tom Jones*. He searches for a structure to explain what is spontaneous and contingent, for "the balance between abstract and particular, type and context, essence and appearance." Here, as in other novels, he makes the narrator instruct the reader in the conventions employed and the states of affairs they illuminate. Fielding, Braudy remarks, teaches us how to understand reality through variety; his narrator is the model historian.

Gibbon—Fielding's admirer, who made a prediction to the effect that *Tom Jones* would outlive the Habsburg monarchy—also offered a method for understanding rather than a thematic interpretation. Sketched in the early *Essai*, pursued in the first three volumes of the *Decline and Fall*, and achieved in the last three is the ambition to make history the conscious creation of a literary object. The esthetic order gains precedence; themes like corruption matter less than literary structuring; relative and open-ended judgments encroach on causal determinants, and long-range causes yield to suggestive perspectives on time and place. Injunctions to the reader and the narrative "I" increasingly obtrude. Gibbon had no commitment to any systematic historical or psychological explanation, "only to his own narrative voice and the coherence it creates."

Braudy prefaces this study of the evolution of a new species of writing history by chapters on Clarendon and Bolingbroke. The treatment

of Bolingbroke is marred by some misconceptions, attributable to its purely textual nature. That of Clarendon is not as original or searching as the account of the three major writers by which this monograph must be judged. The novelty of Braudy's approach—particularly for historians—marks a distinct advance in historiographical study. Since that study has often suffered from biographical reductionism, the reader will note with relief that Braudy ascribes nothing about Fielding to his politics or about Gibbon to the Enlightenment. Yet some readers may feel uneasy about Braudy's textual asceticism and may spot some narrative procedures that owed less to literary experimentation than to some convention or circumstance in the writer's career. But even they are likely to be swayed by Braudy's persuasive reasoning and effective diction. As a negative tribute to the quality and finish of the work, let it be noted that the only perceptible irritant seems to have been contributed by the publisher: the supererogatory practice of putting elision marks before quotations commencing with a lower-case letter.

GEORGE H. NADEL  
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ÁGNES R. VÁRKONYI. *A pozitívista történet szemlélet.* [The Positivist View of History]. (Stúdium Könyvek, Number 67.) Budapest: Gondolat Kiadó. 1970. Pp. 217. 20. F.

The study under review appeared in a series of monographs aimed at the interested layman, which deal with a variety of subjects from philosophy through the natural sciences to the fine arts and literature. A dozen, or one-sixth, of the titles published so far focus on different aspects of history: included are two works by V. Gordon Childe on the dawn of European civilization and the prehistory of European society; one by Federico Chabod on Italy between 1918 and 1948; and several contributions by noted Hungarian scholars, such as the histories of Bulgaria (Emil Niederhauser), Poland (József Perényi), and the Hungarian village (Kálmán Eperjessy).

Ágnes Várkonyi's study is a somewhat abridged version of the introductory chapter of a forthcoming major work that proposes to describe the genesis of and different trends in

Hungarian positivist historiography (p. 213). The author, who has written extensively on the social movements of seventeenth- and eighteenth-century Hungary and who is an expert on the anti-Habsburg struggle led by Francis II Rákóczi, has revealed her interest in the impact of positivism on Hungarian "bourgeois historiography" in two articles, the English versions of which appeared in *Acta Hungarica*. One of these analyzes the echo of H. T. Buckle's *History of Civilisation in England* (1963), and the other the impact of scientific thinking on Hungarian historical writing in the mid-nineteenth century (1968); along with the present booklet they anticipate the opus yet to come.

Writing from a Marxist point of view, Várkonyi's sophisticated approach to positivism is refreshingly different from the standard Soviet evaluation represented, for example, in the relevant article of the *Great Soviet Encyclopedia* (2d ed.; 1955), which still insists, with due reference to Lenin's *Materialism and Empirio-criticism* (1909), that fighting positivism is an important part of the overall struggle against bourgeois ideological influences. As a partial explanation for this difference one may refer to the peculiarities of the Hungarian historical context: between the two world wars official historiography, influenced by German *Geistesgeschichte*, attacked the representatives of positivism from the right equating their activities with mere data collecting. Várkonyi, however, points out that positivist historians followed a well-considered method, the aim of which was not the accumulation of data per se but the detection of the laws of history by a judicious adaptation of the methodology of the modern natural sciences (pp. 17, 85-90). Moreover, she confines herself to the writings of the great nineteenth-century positivists, concentrating on their differing and changing interpretations of the meaning of history. This sober demarcation of her goal has a twofold advantage. On the one hand, it enables her to avoid getting involved in the much debated and debatable issues related to the more recent school of logical positivism and, on the other, it helps her stress the essential without losing sight of important nuances.

Várkonyi draws a sharp line between Marxism and positivism in her analysis of the major historical writings and theories of Auguste

Comte, J. S. Mill, H. T. Buckle, J. W. Draper, W. E. H. Lecky, Herbert Spencer, Émile Littré, and Hippolyte Taine by stressing the common roots of widely differing positivist tenets, namely, philosophical idealism, agnosticism, and a mechanistic methodology (p. 14). But having paid her respect to Marx, Engels, and Lenin, she claims that antidogmatic Marxist historiography must go beyond citations taken from the classics in order to show that the champions of positivism appreciated the first great results of the natural sciences and came "from the ranks of an intelligentsia imbued with liberal and democratic aspirations. In several countries, e.g. Latin American countries, Russia, Poland, Italy, Romania, Serbia, and elsewhere, and not least in Hungary, many progressive people were attracted by [positivism's] ideas" (p. 16). Várkonyi also emphasizes that every place where the remnants of feudalism prevailed in the mid-nineteenth century, the antifeudal tendencies and rational elements contained in positivist thought tended to reflect those interests of the bourgeoisie that pointed in the direction of social progress (pp. 17, 185). To be sure, these progressive features of positivism began to fade away, in Várkonyi's opinion, after the shock of the Paris Commune in 1871 as indicated by the later works of Taine and Spencer who by that time intended to defend established bourgeois society against the movements of the proletariat (pp. 38-41, 84, 120-21, 128-29, 186-89). Obviously the author prefers revolutionary to evolutionary theories; this is also why she attempts to relate the three major phases in the development of positivism to the revolutionary years 1830, 1848, and 1871 (pp. 123-25). Yet it is legitimate to view intellectual trends against the background of the social, national, and scientific revolutions of the age, and Várkonyi's interpretation is far from simplistic: it pays due attention to the variety of emphases in positivist writings, crediting them with the raising of some basic questions about the meaning of history and the introduction of the idea of historical progress into bourgeois historiography (pp. 145-47, 211-12). Noteworthy, also, is the effort to summarize, at least in brief, the lasting impact of positivism on thinkers and writers in Central and Eastern Europe. A reference such as the one made in connection with the third Hun-

garian edition of J. S. Mill's *On Liberty* in 1943, which was a symbolic protest against fascism at the time, goes as long a way toward the re-establishment of much needed intellectual intercourse among scholars of different nations, as does the renewal of reverence for the rational traditions of even antirevolutionary schools of thought (pp. 205-12). Respect for intellectual achievements and recognition of the right to err are, after all, preconditions of meaningful dialogues.

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GEORGE LICHTHEIM. *Imperialism*. New York: Praeger Publishers. 1971. Pp. vii, 183. \$7.50.

George Lichtheim, whose works on Marxism and the history of socialism enjoy a well-deserved reputation, has made an interesting contribution to the study of imperialism. Lichtheim is a Marxist of the left-Hegelian Frankfurt School, and his book may be seen as a useful interaction of both Hegelian and Marxist lines of thought. Unlike the upholders of the Lenin theory he understands that the imperial relationship can exist apart from a capitalist context, and he appreciates, as a Hegelian, the critical role that state power and national consciousness play. At times his primary purpose in writing seems to be that of disabusing Marxists of all shades—a rather substantial number of people these days!—of “crude” economic preconceptions.

The brief volume presents a stimulating but inevitably uneven survey of Western imperialism. A Hegelian emphasis on state and national pride dominates Lichtheim's view of empire from Rome through the Middle Ages, and, perhaps because of Richard Koebner's influence, Lichtheim chooses to discuss such questions as the *Kaiseridee* of the Holy Roman Empire rather than Venetian merchant imperialism or, earlier, the “usurious” imperialism of Rome. The stress shifts to economics beginning with Lichtheim's analysis of the maritime empires of mercantilism. Lichtheim sees, as Hobson or Schumpeter did not, the reality of an imperialism not merely atavistically associated with but erected solidly upon a free trade base. But, unaware of Wakefield's theories of capitalist imperialism in the 1830s and of the role of

classical economics in creating an imperial “ideology,” he views the 1880s as witnessing a “revival,” following an anti-imperialist period, of preindustrial imperialism.

In his final chapters Lichtheim dissects the Lenin theory, to the advantage of Karl Kautsky and Rosa Luxemburg, demonstrating that it does not work in today's world, and depicts a “populist” Maoism, which is more “revolutionary nationalism” than “authentic socialism.” One misses an attempt to deal with psychological or psychoanalytical theories of imperialism—that of Mannoni, for example—more especially since Lichtheim restores *hubris* to a role in political affairs. While Lichtheim does not attempt a theory to rival the classic formulations, it was not asked even of Hercules to do more than cleanse the Augean stables.

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*Anarchici e anarchia nel mondo contemporaneo: Atti del Convegno promosso dalla Fondazione Luigi Einaudi (Torino, 5, 6 e 7 dicembre 1969).* (Fondazione Luigi Einaudi, “Studi,” Number 11.) Turin: the Fondazione. 1971. Pp. 654. L. 6,000.

This book contains the proceedings of a symposium on anarchism held in Turin in December 1969. The goal of the symposium was to give the terms anarchism and anarchy as broad a meaning as possible, to include all antiauthoritarian thinkers and movements from Jean-Jacques Rousseau to Daniel Cohn-Bendit and other student protesters of the late 1960s. Naturally this effort met with some resistance from orthodox anarchists who wanted to exclude everybody else and from libertarians close to anarchism but who refused the label. The participants were of three types: internationally known social and political historians, scholars specializing in the anarchist movement and for the most part anarchists themselves, and veteran fighters from that movement. Each of the six sessions consisted of a paper on a general theme, three to five papers on various aspects of this theme, and a discussion. The six general papers were: “The Genesis of Anarchism in the Nineteenth Century” (Leo Valiani), “Problems in Spanish Anarchism” (Aldo Garosci), “The Contemporary International An-

archist Movement" (Gino Cerrito), "Anarchism between Communism and Individualism" (James Joll), "Anarchism and Bolshevism" (Arthur Lehning), and "Traditional Anarchist Thought and the Contemporary Revolt of the Young" (Jean Maitron). Although the main focus of the symposium was on Europe, with some attention given to Latin America, it is regrettable that virtually nothing was said about the anarchist tradition in the United States; Emma Goldman was mentioned only in passing and there was no discussion of Henry David Thoreau, the Industrial Workers of the World, or the anarchist wing of the North American New Left.

Of particular interest is Cerrito's paper, with its sixty-two-page bibliography. Cerrito claims that anarchism was put on the wrong track by Prince Peter Kropotkin's utopianism and has had to fight its way back to its essential character as an activist movement championing the victims of oppression *à la* Michael Bakunin and Enrico Malatesta. Kropotkin, according to this view, believed that a completely harmonious and libertarian society would come about through an instantaneous, all-encompassing revolution. The anarcho-syndicalists of the pre-1914 era tried to revive the preparation of a working-class elite for the more specific and limited task of destroying the state through political action, especially a general strike. But the First World War drastically weakened the illusion of a self-sufficient working-class movement. Anarchism was further weakened by the rival attraction of the Bolshevik Revolution and by repression from communists and fascists during the interwar years. The most notorious example of such repression came during the Spanish Civil War. Unfortunately, says Cerrito, this event helped revive the myth of an instantaneous, all-embracing revolution even among the practical-minded leaders of the Confederación Nacional del los Trabajadores. (This charge of impracticality was vigorously denied by the Spanish anarchists present.) For the period since the Second World War Cerrito's survey mentions a multitude of minuscule continuing and occasional groupings ranging from the numerous branches of the Movimiento Libertario Español to exiled anarchists from Bulgaria and Cuba to Yiddish-speaking anarchists in the United States and Argentina.

James Joll and Jean Maitron maintain in their papers that the New Left libertarians in many parts of the contemporary world are imbued with the traditional spirit and tactics of anarchism and consequently cannot escape its dilemmas. These dilemmas include communism versus individualism, violence versus peaceful persuasion, and rationalism versus irrationalism. Another dilemma, the insistence on both doctrinal purity and individual judgment, leads to splits and divisions and a shrillness typical of self-righteous ideologues. Lack of direct access to the mass media poses a newer dilemma to those many anarchists who hope to educate the masses along libertarian lines. The bad press their more violent activities get from the media—in both communist and capitalist countries—can draw attention to them, but it can also turn potential sympathizers against them.

In addition to its substantive papers this book has much of value for future researchers on anarchism. There are up-to-date bibliographical references and information on work in progress on many aspects of the movement. For example, A. William Salomone reported that a graduate student at the University of Rochester was filling a gap (noted by Joll) in our knowledge about a group of young libertarians in the German Social Democratic party during the 1890s; Eric Hobsbawm said that someone at the University of London was studying the Cuban tobacco workers during the 1920s. There is a short piece about a new Max Nettlau Library on libertarian movements, which opened its doors in December 1969 in the city of Bergamo. Finally, much of this book itself is a document on the anarchist temperament and the anarchist way of viewing the world. Here the libertarian optimism of Kropotkin lives alongside the bomb throwers and the pessimistic nihilists. Not only do all anarchists hate authority and the state in all its forms, but some, like Pier Carlo Masini, argue that anyone who seeks political power must be sick. Surely there is more to the matter than that.

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MAURICE MANDELBAUM. *History, Man, & Reason: A Study in Nineteenth-Century Thought*. Balti-



more: Johns Hopkins Press. 1971. Pp. xi, 553. \$15.00.

The subtitle of this book—*A Study in Nineteenth-Century Thought*—provides a better description of its contents than the title, *History, Man, & Reason*. The early parts of the work in which the author justifies his undertaking by asserting that thinking in the twentieth century is still widely determined by the notions and conceptions of the nineteenth century is interesting, stimulating, and a healthy corrective to those views which assume that, with the twentieth century, an entirely new intellectual world opened up.

In analyzing the thought of the nineteenth century the author focuses on three different strands of nineteenth-century thought, and this approach explains the book's title. In a first section the author is concerned with historicism. In a second section he deals with "the malleability of man," and this section discusses evolutionist, biological, and psychological theories: the central issue of this section is formed by the question whether conditioning can change human nature. The third section is concerned with the revolt against reason rather than with a defense of reason; the author discusses the positivistic rejection of all non-empirical knowledge and Kierkegaard's and Nietzsche's denunciations of rationalism.

Although the sharp separation of these three trends of thought leads to some overlapping and repetition because certain thinkers—for instance, Hegel—must be treated in the section on "History" as well as in the section on "Man," this form of organization has its advantages because it reveals interesting connections. For instance, I might mention that the philosophical context in which Helmholtz and Mach are placed is somewhat surprising but fully convincing.

The author's detailed knowledge of nineteenth-century thought is admirable. Nevertheless, I have certain reservations. He defines historicism as "the belief that an adequate understanding of the nature of any phenomenon and an adequate assessment of its value are to be gained through considering it in terms of the place which it occupied and the role which it played within a process of development." This seems to me an extremely broad definition, which, I think, allows no distinction between

"history" and "historicism" and which underplays what seems to me the crucial problem of historicism: the impossibility of assuming the existence of generally valid moral norms. Furthermore, the presentation is very abstract; and the criteria that the author uses are primarily of a logical nature. I doubt that this approach does justice to the views of natural scientists; their theories grew out of scientific research and were tentative insofar as they would be changed if further research led to new and different results. Finally, the conclusion of the book is very different from what the reader expects. After having emphasized at the outset that our thinking is dependent on nineteenth-century thought the author simply states that he believes "that the nineteenth-century views of the intellect which have here been examined will, before long, be unacceptable." He does not enter upon any further discussion of the possibilities of survival of nineteenth-century intellectual thought, but the main thesis of the book seems to be that the author shifts the break with nineteenth-century thought from the beginning of the twentieth century to a later period.

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WILLIAM J. BRAZILL. *The Young Hegelians*. (Yale Historical Publications. Miscellany, Number 91.) New Haven: Yale University Press. 1970. Pp. 305. \$10.00.

DAVID MCLELLAN. *The Young Hegelians and Karl Marx*. New York: Frederick A. Praeger. 1969. Pp. ix, 170. \$8.50.

These works on the Young Hegelians (David Friedrich Strauss, Bruno Bauer, Arnold Ruge, Friedrich Theodor Vischer, Ludwig Feuerbach, and others) fill a gap in intellectual history and also contribute to a better understanding of the formative years of Marx—and Engels, too. Professor Brazill, after examining the milieu in which the Young Hegelians emerged and functioned, analyzes their individual careers and thoughts during both the short period when they represented a collective force that influenced their contemporaries (including Marx) as well as in the following years when each of them went his own separate way, often in a more conservative direction. Professor McLellan's book restricts itself mainly to a

study of the thought of each Young Hegelian and to the influence that it had on the development of Marx's views during the period when the Young Hegelians still operated, more or less, as a cohesive group.

The very intensity with which Marx and Engels later attacked the Young Hegelians offers the best proof that they regarded their former associates as a force to be reckoned with. Their first two joint works, *The Holy Family* and the major part of *The German Ideology*, were designed to discredit "Bruno Bauer and Consorts" lest others continue to pray in a shrine where they themselves had once worshipped. It should be clear, therefore, that any evaluation of the judgment of Marx and Engels in this connection must be defective in the absence of the more thorough and separate consideration of the Young Hegelians as such that Professors Brazill and McLellan offer. Simultaneously, the role of the Young Hegelians in discrediting the old order and above all the validity of traditional Christianity is clarified. If a contemporary generation occasionally has proclaimed the death of God, the Germans of the 1830s and 1840s read a few similar pronouncements.

Both authors show a certain reluctance to link Marx too closely with the thoughts and especially the activities of the Young Hegelians. Thus they fail to bring out clearly that Marx's interest in a professorial chair in Bonn perhaps was subsidiary to his desire to join Bruno Bauer there in an atheistic crusade. An equal vagueness is apparent in connection with Marx's earlier role in the policies of the *Rheinische Zeitung*. He accepted, among other things, responsibility for the selection of Adolf Rutenberg, a Young Hegelian from Berlin, as the second chief editor of the paper in 1842. In tracing the impact of the Young Hegelians on Marx Professor McLellan notably confines himself too much to the search for specific ideas and concepts that the latter may have derived from the former. The letters that he consulted certainly suggest that Marx in the course of several campaigns bivouacked frequently with Bruno Bauer and Arnold Ruge. But a cautious approach is justified in many other instances, as when he cites certain concepts that Marx may have adopted from specific Young Hegelians, notably Max Stirner. McLellan states (p. 136) that Stirner's book was "to a large extent

an amalgam of current clichés . . . the ideas of alienated labour and exploitation were by no means confined to Marx at this time, even among the Germans."

Professor Brazill, in citing a special study on the subject, underrates the obstacles that German writers faced under the Carlsbad Decrees of 1819. He states that everything "less than twenty pages in length" was subject to censorship (p. 83). The decrees actually said twenty "Bogen" (printer's sheets of 16 pages each, namely 320 pages). A fuller identification is in order when he quotes the memoirs of a member of the Frankfurt Parliament to the effect that "next to Freiligrath, Feuerbach was the most silent man there" (p. 152). Otherwise the reader immediately thinks of Ferdinand Freiligrath who, following various vicissitudes in 1848, became what one might call the poet laureate on the editorial staff of Marx's *Neue Rheinische Zeitung* from 1848 to 1849. But these are minor lapses that detract little from the broad merits of the work.

The texts of Professors Brazill and McLellan are very readable and rest on a reasonably thorough investigation of the primary sources as well as secondary studies. If Professor Brazill appears somewhat repetitious, this reflects the difficulties inherent in the writing of successive accounts of the views of various Young Hegelians who had certain things in common. The documentation, bibliography, and indexes in both books certainly are adequate, though not painstakingly exhaustive.

OSCAR J. HAMMEN

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JOHN STUART MILL and HARRIET TAYLOR MILL. *Essays on Sex Equality*. Edited and with an introductory essay by ALICE S. ROSSI. Chicago: University of Chicago Press. 1970. Pp. ix, 242. Cloth \$8.75, paper \$1.95.

It has not escaped notice that the sacred text of the Women's Lib movement, "The Subjection of Women," was written by a man. By extending the canon to include "Enfranchisement of Women," the editor of the present volume has been able to issue it under the dual authorship of John Stuart Mill and Harriet Taylor Mill. ("Enfranchisement of Women" had originally been submitted for publication as Mill's own work but was later attributed by him to his

wife, his own share in it, he explained, being "little more than that of an editor and amanuensis.") The two also figure as the dual heroes of Alice Rossi's introductory essay, "Sentiment and Intellect," in which their marriage is represented as the near-idyllic relationship that approximates the "dream in the heart of many young women searching for liberation in 1970."

The nineteenth-century idyll unhappily falls rather short of the twentieth-century dream. Professor Rossi sees its only shortcoming in its denial of sexuality, apart from which she finds it an admirable union of sentiment and intellect. (Not, needless to say, Harriet's sentiment and John's intellect. It would be cruelly ironic if this were the meaning attached to the title of her essay.) Other Mill scholars might find it hard to understand how this vision of the marriage can survive a reading of Friedrich Hayek's edition of their correspondence, from which Harriet emerges as a person without intellectual distinction and notably deficient in sentiment—indeed as being uncommonly vain and overbearing, mean-spirited and small-minded.

One would like to be able to put aside personalities and judge this volume in terms of the very considerable issues raised by the subject itself. Yet personalities constantly intrude and not only in the introduction, which so inflates the intellect and elevates the character of Harriet Taylor. This first essay is followed by an exchange of views on marriage and divorce written by Mill and Harriet Taylor shortly after they met apparently for their mutual edification. Here, too, one cannot resist comparing their respective contributions—twenty pages by Mill of rational, lucid reasoning, and four pages by Taylor of lofty, romantic, inchoate sentiments, uninhibited by conventional syntax or logic. (In printing the latter, Rossi proves herself a better scholar than "Women's Libber," since it is just this kind of prose that confirms the worst stereotype of the "feminine mind.") Nor can one help comparing Harriet Taylor's "Enfranchisement of Women" with Mill's "The Subjection of Women," again to Mill's favor.

The theme of Mill's essay has become familiar enough in recent years. (The complete essay is available in several editions, and excerpts ap-

pear in numerous anthologies.) The subjection and subordination of women, amounting, Mill claimed, to a virtual condition of slavery, was typified in the marriage contract, which gave the husband control of his wife's property and person and made him the guardian of their children. It is sometimes said that the abolition of these legal disabilities has made Mill's thesis obsolete. But this does not take into account his subtle view of subjection, in which the victim may become an unwitting, or even a witting and willing, collaborator in her own abasement. Nor is his argument vitiated by the fact that men have often chosen to exercise their power benevolently, since it is the fact of power rather than its use or abuse that he found, and many still find, degrading.

But if we are to take the essay as a tract for our times, we must also consider another aspect of it, which might give pause to some, particularly among academics, who have so heartily endorsed it. For Mill was arguing not only for a policy of equality but also for a policy of liberty. It was equality of opportunity that he was seeking, which meant, for him, the open, unlimited freedom to compete. The demand for equal or even proportional representation in a faculty or governing body, for fixed quotas at various ranks, to say nothing of "discrimination in reverse," would have been utterly abhorrent to him. His position here, as in *On Liberty*, was thoroughly individualistic; it was not the corporate body of women—their corporate rights, identity, or consciousness—that concerned him, but the rights, identity, and consciousness of the individual person. And even more than in *On Liberty* (perhaps because it was written later), his argument was Darwinian: the free play of competition was necessary to ensure the survival and ascendancy of the fittest—the fittest individuals, whether men or women.

As with all sacred texts, this one has great potentiality for heresy.

GERTRUDE HIMMELFARB

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LEWIS H. GANN. *Guerrillas in History*. (Hoover Institution Studies 28.) Stanford: Hoover Institution Press, Stanford University. 1971. Pp. vii, 99. \$3.95.

This is a fine expanded essay on modern guer-

rillas and insurgency. The materials on ancient and medieval slave, peasant, and popular uprisings are too sketchy to be helpful; those that deal with nineteenth- and twentieth-century insurgency and counterinsurgency are excellent. In an essay of this kind an index and an annotated bibliography would have been most useful; the Hoover Institution Press has not had much experience in publishing materials of general military educational interest. The most obvious omissions in Dr. Gann's bibliography are C. E. Callwell's *Small Wars: Their Principles and Practice* (1899) and Wilhelm Rüstow, *Die Lehre von Kleinen Kriege* (1864). That there is no notice of the export of European-mounted rural constabularies as one feature of modern imperialism seems surprising in view of Dr. Gann's interest in colonial history. On the whole, however, this is a thoughtful and well-balanced work. Its tone is one of Clausewitzian respect for the chameleon-like nature of human conflict, and it is quite free of morals and maxims from other species that make some works on war as unhelpful as the experiment with George Smith's dog Dewey, who, instead of getting fits from his failures, proved to be "profoundly incurious about the puzzle."

THEODORE ROPP  
Duke University

RAYMOND LISTER. *Antique Maps and Their Cartographers*. [Hamden, Conn.:] Archon Books. 1970. Pp. 128. \$8.50.

Raymond Lister's new book on the history of map making, following his earlier work, *How to Identify Old Maps and Globes* (1965), is an attempt to present a short survey of the art and science of map making to the general public. Such an addition to the literature of the subject would indeed be valuable; unfortunately, the manner in which the author organizes his material tends to leave the reader confused rather than informed.

Lister presents the information he amassed in three different ways. Some of it is organized in a straightforward chronological manner; some of it is described under the headings of the principal schools of cartography, for example, Low Countries, France, Great Britain; and some information is provided under regional divisions, for instance, the cartography of Africa, Asia, and Australasia. In the process,

some information is conveyed in a sketchy manner; for example, the presentation of Chinese cartographic traditions or the chapter on map making in America, which omits any mention of such pre-Columbian traditions as were preserved in Aztec codices.

While there are several misspellings and a few notable errors in the book, it does nonetheless render service to the general public in two ways. First, its bibliographical sections are truly useful, not pretending to be complete yet providing basic references and a number of detailed studies that clarify the topic under discussion. And second, those sections of the work that deal with British map making contain a sizable body of biographical data on British cartographers, and information on their work, that would have to be laboriously gathered from a number of scattered sources.

A final note on the illustrations seems to be in order. The book contains fifty-eight good full-page photos of maps and a handsome color reproduction on the dust jacket. Yet, while fully recognizing the exceptional importance and width of range of the collections of maps held by the British Museum, I cannot but regret the scanty representation of remarkable maps held in other collections, outside the United Kingdom. This lack is especially noticeable when it comes to specimens of map making prior to 1500 and to specimens of non-European cartography.

GEORGE KISH  
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OTTO MAYR. *The Origins of Feedback Control*. Cambridge, Mass.: M.I.T. Press. 1970. Pp. vii, 151. \$7.95.

OTTO MAYR. *Feedback Mechanisms in the Historical Collections of the National Museum of History and Technology*. (Smithsonian Studies in History and Technology, Number 12.) Washington: Smithsonian Institution Press. 1971. Pp. x, 133. \$3.25.

Mayr begins *The Origins of Feedback Control* with a definition of feedback drawn from that given in 1951 by the American Institute of Electrical Engineers. Armed with his definition he seeks in the past mechanisms that fit it and that subsequently form the raw data he uses to construct his chronological narrative. He opens

his narrative with the float valve that was discovered in antiquity, that drifted out of sight during the thirteenth century with the demise of the water clock, and that finally reappeared in the eighteenth century. Temperature regulators, as he recounts, appeared during the seventeenth century in the laboratories of scientists, but the regulators did not become practical devices until the early part of the nineteenth century. The search for efficient safety valves added pressure regulators to the technology of steam engines. The rising use of windmills during the eighteenth century fostered the development of feedback control mechanisms, the most notable of which, the rotating pendulum, inspired the realization of the steam engine governor, so he tells us. After discussing an instrument that permits the clocksmith to regulate clocks accurately, he closes his narrative. In a few final pages he makes an interesting comparison between the rise of liberalism and that of feedback control.

The book *Feedback Mechanisms* amplifies and broadens the treatment given in *The Origins of Feedback Control*. In the former book, which follows the same approach as that followed by the latter, the author chronicles the various types of feedback mechanisms found at the National Museum of History and Technology, thereby realizing a catalog that reveals the richness of the collection of instruments and machines found in this museum.

Mayr's bibliographical references show that he researched extensively in the preparation of his two monographs. In them the historian will find useful details but will look in vain for themes or arguments that would help him understand what factors led to the discovery of feedback controls and to their evolution. A definition drawn from engineering practice of the mid-twentieth century constitutes a poor start for a historical study because it leads to "whig history," to a natural history of technology. Mayr's book reads the way treatises on taxonomy did before the publication of Darwin's *On the Origin of Species*.

ROBERT M. MCKEON  
Tufts University

C. D. O'MALLEY, editor. *The History of Medical Education: An International Symposium Held February 5-9, 1968*. (Sponsored by the UCLA

Department of Medical History, School of Medicine; Supported by the Josiah Macy, Jr. Foundation. UCLA Forum in Medical Sciences, Number 12.) Berkeley and Los Angeles: University of California Press. 1970. Pp. xii, 548. \$20.00.

This is a very venturesome book. Sweeping from ancient times to the present, it tells the history of medical education in nearly every part of the world (with Canada, Australia, and most of Africa being the only major areas omitted). A collaborative effort, the volume brings together articles by nineteen scholars who attended a 1968 international symposium on medical history at UCLA.

Ambitious though it is, the book is not, on the whole, very successful. *The History of Medical Education*, to me, is flawed in two important respects. Most serious is the lack of any introductory or other editorial comment tying the various (and numerous) selections together into some semblance of a unified whole. A single title page is the only guide one finds to the book's four major sections ("The Earlier Period in the West," "The Modern Period," "Eastern Europe and the Far East," and "Western Hemisphere"). Admittedly, writing such an introduction would be tough going, given the diversity of the nineteen selections; but that does not reduce the need. It magnifies it. Not having editorial comment, one feels like a man who was expected to reassemble a bag of parts into the original machine without any blueprint to guide him. Such an introduction might have noted the historic contribution of rulers and national governments to medical progress, the frequent reform role of medical students, the anticipation of modern medical advances in earlier periods, and the influence of Islamic and Western medicine (chiefly in Renaissance Italy, the Netherlands, Scotland, and Germany) on other national systems.

The other shortcoming, less serious because it is not generally characteristic, was the failure of several authors to consider relevant social and cultural factors. More contributors might profitably have heeded their colleague, William F. Norwood, who suggested that medical historians follow the approach of the physician, who always "examines the history as well as the physical condition of the patient, including his total environment." Instead, several authors wrote as if medical education developed



in a social vacuum, unaffected by the wars, politics, social needs, and cultural forces that beat against medical school walls. As a result their articles were too much a tale of who taught what, when, to whom, and by what means. Into that category fell the selections on Classical Antiquity, the Middle Ages, Italy after 1600, the Netherlands, Scotland, India, and Ibero-America (although there, the concluding portion did note the influence of external factors).

Other selections, however, took a much broader view, and a few were solid historical accounts. I found the selections on Japan, Russia, the U.S. to 1900, England, and France very rewarding. The piece on Russia, besides pointing up the advances during the Tzarist period, reminded us again how far ahead of us the Soviets are in providing adequate medical care for all citizens. Other useful articles are those on twentieth-century American medicine, valuable chiefly as a bibliographic essay and on Indian and Islamic medical education, which forcefully (perhaps too forcefully) presented the claims of those regions to one-time world leadership.

*The History of Medical Education* will prove useful and interesting to cultural historians and historians of science and medicine, provided they read it selectively. Overall usefulness, however, is pretty much limited to reference purposes. One simply cannot read this book through and come away with any sense of a unity in the history of medical education on a worldwide basis.

EDWARD H. BEARDSLEY  
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JOHN R. PAUL. *A History of Poliomyelitis*. (Yale Studies in the History of Science and Medicine, Number 6.) New Haven: Yale University Press. 1971. Pp. xv, 486. \$15.00.

The history of disease has always interested medical historians, and writing the entire history of a single disease is an immensely challenging enterprise. Rarely has the task been carried out flawlessly, because it is difficult for one man to encompass with equal competence a span reaching, for instance, from Imhotep to Jonas Salk. Dr. John R. Paul (1893-1971), who for many years was professor of preventive medicine and epidemiology at Yale, has accom-

plished the feat better than most. In forty-two chapters he tells the story from ancient evidence of the crippling effects of polio through the modern recognition of its viral nature, its epidemiological and serological characteristics, and its growth on tissue cultures, to the effective means of immunization now used.

Though an ancient disease in all probability, only in the last hundred or so years has polio been recognized in widespread epidemic form. We now know that it is an intestinal as well as a neurological disease and that it is endemic in the less highly sanitized areas of the world, where most infections are unapparent. The development of the iron lung, the controversy over Sister Kenny's treatment with warm packs, the avoidance of immobilization of limbs, and the scientific excitement of the 1954 field trials with Salk vaccine followed by the tragic events of the Cutter-produced vaccine of the following year are beautifully told.

Some negative criticism, however, must be made. Because in two books of the *Epidemics* of Hippocrates no cases of polio are included, Dr. Paul therefore assumes that the disease existed in antiquity only in sporadic form. In the first place, the argument *ex silentio* is weak and, furthermore, of the seven books of *Epidemics* that have survived, only two have been translated into English. I hasten to add that Dr. Paul makes no pretensions about being a historian; yet he has written a book of history, so one is put on guard. More than half the book is devoted to the last thirty-five years, and quite justifiably so. Here one might question his relationship to the National Foundation for Infantile Paralysis, which never seems to have been close. At times he sounds a bit querulous, and he has failed to develop the theme of private philanthropy and basic medical research, which is central to the story of poliomyelitis. Perhaps Dr. Paul may be excused from the latter charge because he has relied heavily on Saul Benison's memoir of Dr. Tom Rivers, and he knew that Benison is at work on a major study of the National Foundation's role.

All historians of twentieth-century medicine, especially of the rapidly developing field of virology, will be indebted to Dr. Paul. He illustrates how a scientist in the midst of events in the recent past sees their evolution and implications. His honesty and modesty are appeal-



ing, but perhaps the latter is overdone. The inscription on a fine photograph of Jonas Salk on page 416 says: "To John Paul—who long pointed the way."

GERT H. BRIEGER  
Duke University

GILLIAN T. CELL. *English Enterprise in Newfoundland 1577-1660*. [Toronto:] University of Toronto Press. 1969. Pp. 181. \$7.50.

A new book on Newfoundland is always welcome to the growing community of students of Canadian history as well as to those interested in the expansion of Europe. This work deals with two areas of English enterprise, the development of the vast fisheries off the Newfoundland coast from 1577 to 1660 and the attempts to establish permanent settlements on the island for both strategic and economic reasons. Her work is based on the port records of the English west country, and for the colony she has discovered papers and a journal of Sir Percival Willoughby, one of the principal investors in the London and Bristol Company for the colonization of Newfoundland. These throw some new light on John Guy, the first governor of the colony and later mayor of Bristol. Since Willoughby was an owner of iron works, Guy reported the discovery of iron deposits but concluded that the colony's economy would be based on timber and fish. This, of course, involved difficulty with the fishermen, who made seasonal journeys to Newfoundland. The company also made grants to five other patentees including Baron Baltimore. Of all these attempts at settlement between 1610 and 1630 Mrs. Cell concludes none "had proved the answer to the problem of how to colonize Newfoundland," although they had proved it possible to live on the island. How many settlers remained is not estimated.

On the fishery itself the work gives only partial results. The impressive attempts at quantification indicate the development of a substantial re-export trade of Newfoundland fish by west country ports and fishermen, but the records are incomplete. On the enforcement of fish-eating legislation, Mrs. Cell differs from Samuel Eliot Morison in believing that it was usually evaded, and her work lacks the precise discussion of shipping and seafaring that the

work of J. H. Parry, Morison, and others has led us to expect. "Sack ship" even if meaningful to Newfoundlanders has little meaning when it is realized that such ships varied from seventy to five hundred tons, nor does she explain how a fishing ship of seventy tons could carry eight dories (p. 4) each weighing three to five tons to Newfoundland. In conclusion this is a useful work with an excellent bibliography but more tantalizing than satisfying in its conclusions because of the nature and scarcity of the sources.

FRANCIS COGHLAN  
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JOHN HOHENBERG. *Free Press/Free People: The Best Cause*. New York: Columbia University Press. 1971. Pp. xvii, 514. \$9.95.

The literature pertaining to freedom of the press is fairly extensive, including such representative titles as Morris L. Ernst's persuasive *The First Freedom* (1946) and Leonard Levy's legalistic *Legacy of Suppression; Freedom of Speech and Press in Early America* (1960). Yet it may well be true that nowhere in print is there as broad-gauged and informing an account of the history of press efforts to criticize the government as the volume under review.

*Free Press/Free People* is the seventh book of John Hohenberg, quondam working journalist in the United States and abroad and presently professor of journalism at Columbia University. His thesis appears to be that there is a "sensitive and little understood relationship" of interdependence between a free press and a free people and that neither can exist very long without the other in any country or any age. To substantiate this viewpoint, the author moves rapidly through history, focusing on such topics as the Zenger case, the struggle for survival of the United States abolitionist press, the contribution of Bismarck to the ancient and dishonorable art of press manipulation, and affirmations of the libertarian creed by John Stuart Mill, Franklin D. Roosevelt, and others. Whereas much of the book deals with the United States, Britain, and Western Europe—where the battle for a free press primarily has been waged—it also treats significantly the press in China, India, Japan, and Latin America.

Although the privately financed Hutchins Commission on the Freedom of the Press reported in 1947 that the current danger to press freedom lay within the press rather than outside, Professor Hohenberg has relatively little to say about the social responsibility of the press. Also there seems to be some contradiction between the quotation (p. 468) from Walter Lippmann about the fragility of an independent press and Hohenberg's vigorous affirmation that the days of the free press are not numbered.

The lack of either a bibliography or any other form of documentation seems unfortunate in the light of several unsupported statements about important issues. How, for example, does the author know that Japanese intelligence never picked up any information about the Chicago *Tribune* disclosure that the United States had broken the Japanese code (p. 258)? And what is his basis for saying (p. 348) that American newspapers had not fallen so low in public esteem for more than a hundred years as after the Kennedy and Oswald assassinations? These reservations aside, *Free Press/Free People* embodies a useful synthesis of the tensions between newspapers and their governments within broad dimensions of space and time.

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Chatham College

BRIAN CHAPMAN. *Police State*. (Key Concepts in Political Science.) New York: Praeger Publishers, 1970. Pp. 150. \$5.00.

This compact study deftly achieves the aim of Praeger's Key Concepts series: to clarify certain "emotive" terms through historical and semantic analysis. Five chapters trace the "police state" from its eighteenth-century origins to recent times; four chapters discuss "new meanings" of the term, police methods and psychology, and the author's conclusions. The analysis rests upon historical data from European states, whose concepts and practices of the police function have differed from those in English-speaking countries where the term "police state" has been used "indiscriminately" as a pejorative since the 1930s.

Chapman discerns three kinds of police states. The "traditional" model evolved within

the bureaucratic structures of the pre-Napoleonic French, Prussian, and Austrian monarchies and reached fruition in Fouché's "state apparat" as protector, censor, and moral guide for society, subject to law and a rational division of administrative labor. After 1815 the traditional police state both continued (symbolized by Napoleon III) and then gradually lost its intrinsic qualities in the legal and institutional reforms preceding 1914. A "modern police state" model arose as Weimar Germany's *Rechtsstaat* yielded to Hitler's system: "It was like using a Rolls Royce not in order to carry passengers in comfort, but to run people down in the street." Nourished by an extraconstitutional taproot of power, the police penetrated the entire state structure and fused once-dispersed powers into a monolithic "offensive force" in society that finally became the director of internal policy. At that point the "totalitarian police state" superseded its "modern" progenitor. Himmler's police apparat replaced the party as the state's ideological and administrative vanguard. Police power proliferated proportionally with the state's authoritarianism; in a state where authority knew no limits the police in time sought to become the state. Such, too briefly, is the author's historical argument.

Very perceptively Chapman relates the problem of police powers to modern society generally. Even in nontotalitarian states police necessarily wield degrees of arbitrary power, so that "the application of the law, although not the law itself, becomes relativistic," and the potential for abuse of power is endemic. Not only legal, but viable social norms shared by the police are necessary restraints. Impatient social idealism applied by reforming zealots bent upon rapid change can spawn a modern police state. We must constantly choose between the police as "either an instrument or a master."

The book is a stimulating challenge to further research and interpretation. One regrets only that the "modern" and "totalitarian" police state types are derived nearly exclusively from the Nazi experience; only passing reference is made to the USSR and none to Fascist Italy.

HOWARD C. PAYNE  
Washington State University

*Mouvements nationaux d'indépendance et classes populaires aux XIX<sup>e</sup> et XX<sup>e</sup> siècles en Occident et en Orient.* In two volumes. (Commission Internationale d'Histoire des Mouvements Sociaux et des Structures Sociales.) Paris: Librairie Armand Colin. 1971. Pp. xvii, 402; 414-715. 100 fr. the set.

KONSTANTIN SYMMONS-SYMONOLEWICZ. *Nationalist Movements: A Comparative View.* Meadville, Pa.: Maplewood Press. 1970. Pp. ix, 91. Cloth \$5.50, paper \$3.50.

The two volumes of the first study are issued with the cooperation of UNESCO, the Institute for Social and Economic History at Heidelberg, and the Volkswagen Foundation. They are published under the auspices of the International Council of Philosophy and the Human Sciences and the National Tunisian Commission for UNESCO. The reports result from a four-year cycle of study in two stages: the first gathering held in Tunis and the second at the Twelfth International Congress of Historical Sciences at Vienna in 1965. Some forty scholars contribute to the synthesis; two-thirds of the reports are in French, one-third in English.

The purpose is to investigate the participation of popular classes in national movements of independence throughout the world and to find possible connections between national and working-class movements. Major sections are devoted to Europe, the Arab countries, black Africa and Madagascar, Asia, Canada, and Latin America. There is a summary for each major section, and six scholars (Labrousse, Portal, Bédarida, Droz, Chesneaux, and Bureau) contribute to the general conclusions.

As is to be expected in studies of this kind, no amount of briefing beforehand can assure common treatment. The result is that synthesizers find difficulty in extracting trends or currents. The varying approaches reflect the diversity of national movements. (Scholars of nationalism rarely find consensus.)

The papers are uneven in value. On the plus side is the brilliant contribution of Eric Hobsbawm (London), who explains why Ireland provides a classic case of nationalism, whereas Scotland and Wales have developed no serious movements for national independence. Equally meritorious is the paper by Werner Conze and Dieter Groh (Heidelberg) on "Working-Class Movement and National Movement in Ger-

many between 1830 and 1871." Of lesser importance are the contributions by historians in underdeveloped countries who cannot resist the urge to continue beating the dead horse of Western imperialism.

The final section is devoted to tentative conclusions on such matters as the existence of a diffuse national conscience, opposition to the foreigner, organic bonds between social struggle and national liberation, attitudes of revolutionary parties to nations, problems of actions between countries, and obstacles to national emancipation.

The contribution by Vladimir G. Trukhanovskii (Moscow) on popular masses in the Soviet Central Asian Republics reveals the sad fact that some Russian historians either do not know or are unwilling to accept the difference between history and historicism. Apparently there is a chasm between historians contaminated by the bourgeois Enlightenment and Soviet scholars blessed with total understanding.

Trukhanovskii opens with what he believes to be a correct interpretation of historical development. He is certain that, notwithstanding all its multiformity and contradictory character, history constitutes a single law-governed process. Mankind passes through five socioeconomic formations: primitive-communal, slave-owning, feudal, capitalist, and communist. The transition from one formation to another is determined not by the free choice of men but by objective laws operating independently of man's will under the principal law of social revolution. "With the establishment of the communist formation, antagonistic contradictions between the production forces and relations of production come to an end, the exploitation of man by man is abolished and the antagonistic classes and the class struggle disappear."

Trukhanovskii exempts the Soviet Central Asian Republics and Kazakhstan from his law of progression. Fortified by an eight-page bibliography from the "Archives," by "Documentary Publications," and by "Articles in Scientific Journals," he tells how popular masses in this area acceded to the socialist socioeconomic system by "by-passing the capitalist stage of development." The general law-governed process is not violated: "individual exceptions merely serve to confirm the general rule."

The Russian historian informs the newly emerged Asian and African states that they, too, "have the good fortune of being able, if they so desire," to obtain Soviet support. He quotes Lenin (1916): "We shall exert every effort to establish cordial and fraternal relations with the Mongols, the Persians, the Indians, the Egyptians, we consider it our duty and our interest to do so."

One turns with a sense of relief to the modest but scholarly study by Konstantin Symmons-Symonolewicz. *Nationalist Movements: A Comparative View* is a welcome addition to the growing literature on nationalism, certainly the finest sociological study since Florian Znaniecki's *Modern Nationalities* (1952). As a sequel to his earlier book, *Modern Nationalism: Towards a Consensus in Theory* (1968), in which he gave a valuable typology of nationalism, Symmons-Symonolewicz presents a comparative analysis of nationalism as a social movement. He seeks to fill a gap by considering nationalism as a social movement. He uses as his focus the factors that underlie the origin of nationalist movements, shape their development, and determine their source or failure.

Historians will be pleased by the author's approach: he is sensitive to nationalism in time and space. Unlike other sociologists he uses language readily understandable by scholars in other disciplines. He knows the enormous literature of nationalism well, and he uses it effectively. He concentrates on essentials, and his interpretations are rational and impressive.

In compact chapters Symmons-Symonolewicz discusses nationalism from cultural crisis to cultural and political self-assertion, national language and national boundaries, nationalism as ideology, the nation as a moral community, the crucible of alien rule, and nationalism in historical perspective. He describes the most common type of nationalism—that of subject peoples striving for cultural and political emancipation—and gives attention to the "life cycles" of European movements. He does not ignore the distinctive features of non-Western nationalism. Like the late Hans Kohn, he is convinced that there never has been and never can be any justification for a policy of national oppression. "So long as national, or ideological, oppression exists there can be no durable peace."

Symmons-Symonolewicz's study is encouraging to historians of nationalism who favor a multidisciplinary approach. It does much to help clarify an ism suffused with paradox, inconsistency, and contradiction. Above all, this first-rate study reveals that there is still merit in the Rankean goal: to tell it "wie es eigentlich gewesen ist."

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BARRY M. GOUGH. *The Royal Navy and the Northwest Coast of North America, 1810-1914: A Study of British Maritime Ascendancy*. Vancouver: University of British Columbia Press. 1971. Pp. xvi, 294. \$12.00.

The northwest coast of North America was so remote from Great Britain and so close to the rising power and population of the United States that it is remarkable that Britain succeeded in holding it. Gough's thesis is that the main explanation lies in the strength of the Royal Navy. No matter how deeply Americans felt about "Fifty-four forty or fight," even President Polk could not overlook British power. The book traces the history of Britain's rule on the northwest coast during about a century, from the time when her fur traders appeared on the Pacific to the transfer to Canada in 1910 of the naval facilities at Esquimalt on Vancouver Island. Other topics of interest deal with the Oregon crisis, minor naval engagements on the Pacific with Russia during the Crimean War, Anglo-American difficulties during some small gold rushes, the dispute over the San Juan Islands, and repercussions in the northwest of the Anglo-American antagonism during and after the Civil War—all, except the Oregon controversy, events of little importance.

Gough naturally tries to make as much as possible of the impact of the navy in determining the outcome of these incidents. As a result he is apt to minimize other factors that may have been more significant. In analyzing the Oregon settlement, for example, he mentions Aberdeen's threat to commission thirty ships of the line and concludes: "In all likelihood, this alarming news induced the Americans to adopt a less belligerent attitude" (p. 79). Perhaps so; but the point is a contested one, and there is

more reason than Gough admits to believe that the thirty ships were not crucial.

Beyond this point of detail there is an overall lack of clarity as to whether the decisive factor was, not British naval operations along the northwest coast, but British sea power in general. It scarcely needs arguing that the fact that Britain had the world's strongest navy threw a shield over British Columbia, just as it did over all British colonies. Much less evident is the contention that the occasional appearance of one or more warships on the coast had any additional effect. Thus the book is primarily a history of British Columbia developing under the protection of a mighty motherland, rather than a history of the specific contribution of naval units operating on the northwest coast.

The book is well written and thoroughly documented. The appendixes give details about ships and officers on the northwest coast. For anyone interested in a fringe area of British sea power, the book is to be recommended.

CHARLES S. CAMPBELL  
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ROBERT A. HUTTENBACK. *Gandhi in South Africa: British Imperialism and the Indian Question, 1860-1914*. Ithaca: Cornell University Press. 1971. Pp. ix, 368. \$9.75.

"The genius of the British constitution requires," M. K. Gandhi wrote in 1911 as he struggled with the South African government, "that every subject of the Crown should be as free as any other, and, if he is not, it is his duty to demand and fight for his freedom so long as he does so without injuring anyone else." It is one of the many curious aspects of this struggle, as of the later and more fateful one in India itself, that Gandhi did not appeal to any primordial rights of man but always stayed within the framework of a legalism that depended upon appeals either to existing British laws or to what he conceived to be the spirit of the British Constitution. This fact is probably more significant for the successes and failures of the movements he led than any insights that may come from the elucidation of his theological and philosophical positions. A principal merit of Robert A. Huttenback's study is that the juridical element is kept to the forefront, a reminder that while the con-

frontations between Gandhi and the bigots are a moving testimony to the dignity of the human spirit, the basic confrontations took place in the law courts and in government offices. It was there that the officials—South African, Indian, and British—who were faced with the political dilemma of liberalism tried to work out a solution for a problem that was insoluble.

The problem was very neatly stated by Sir West Ridgeway, a former Indian governor, in the inevitable letter to the *Times*. He noted that the people in England who wanted all citizens in the Empire to enjoy equal rights were at the same time believers in the Empire—"academic imperialists" he called them (p. 332). With brutal clarity he pointed out that it was precisely the self-governing colonies—that is, the ones run by the whites—that would never tolerate the entry of blacks in any number and certainly would never give them equal rights. There is a special poignancy, then, in following Gandhi's quest for equality before the law for his fellow Indians in South Africa, for one senses that he really believed that to appeal to the law was to reach beyond the oppressors to a source of freedom.

Huttenback has written a worthwhile book, but like the rest of the works that have sought to illumine Gandhi's place in history, it depends to a very considerable extent upon our prior acceptance of Gandhi's greatness. We are told what Gandhi did, and how people responded, but not much about why he acted, or why he engendered the response he did. As Gandhi recedes in time and public memory his life and times become more enigmatic, less susceptible to the methodology of political history.

As a comment on the continuing lack of academic communication despite all the efforts of recent years, it may be noted that B. N. Mishra completed a doctoral dissertation for Patna University in 1969 on this same topic of Gandhi's South African years. Reference to it probably would not challenge Huttenback's general emphasis, but it adds to the understanding of the issue by greater attention to its importance for Indian politics.

AINSLIE T. EMBREE  
*Duke University*



ARNOLD THACKRAY. *Atoms and Powers: An Essay on Newtonian Matter-Theory and the Development of Chemistry*. (Harvard Monographs in the History of Science.) Cambridge, Mass.: Harvard University Press. 1970. Pp. xxiii, 326. \$12.00.

The evolution of science immediately before and after the development of Newtonian physics has frequently been described as the gradual refinement of a powerful analytical approach involving reductive descriptions of natural phenomena and the quantification of forces. Seen from this standpoint, scientists of the sixteenth, seventeenth, and eighteenth centuries were arriving at the "mechanization of the world picture," a phrase recently considered to be synonymous with the emergence of modern science. Historians have occasionally described the work of such scientists as Lavoisier and Dalton as a "delayed revolution," indicating the belief that the introduction of the mechanical view came later in chemistry than in physics.

Arnold Thackray, who teaches the history of science at the University of Pennsylvania, has presented a different view of the development of chemistry. In this impressive and sensitive monograph in the Harvard series on the history of science, Thackray demonstrates that the triumphs of nineteenth-century chemistry were not built on a reductionist foundation but rather were achieved in isolation from the science of physics.

Thackray's thesis is that the descriptive model used by the partisans of the new chemistry rested on a different concept of matter than that of the physicists in the Newtonian tradition. The physicists were enamored with the measurement of micro-scale forces of attraction and wished to explain chemical reactions in terms of a mathematical law that would do for "chemical mechanics" what Newton's inverse-square law had done so brilliantly for celestial mechanics. Furthermore, many of them accepted Newton's "nut-shell" theory of matter, which assumed both the inertial homogeneity of matter and the inaccessibility to observation of its most elementary constituents. These assumptions were unproductive and even obstructive when applied to chemistry; progress in that field came only when these concepts had been replaced by a nonreductive emphasis

on macro-scale weight studies, the assumption of the inertial heterogeneity of matter, and a definition of elements that rested on the results of laboratory analysis rather than matter theory.

Thackray has not only revealed the internal characteristics of rival conceptual schemes of chemical structure but has also attempted to illustrate the links between these schemes and theology, philosophy, and developing industry. In a book of modest size no one could entirely succeed in the latter assignment; Thackray, realizing this limitation well, has written an unusual conclusion that is more a call for additional research than a summary of his own work. Both the achievement of the book and the invitation in its conclusion merit attention.

LOREN R. GRAHAM  
Columbia University

THOMAS HAWKINS. *Lebesgue's Theory of Integration: Its Origins and Development*. Madison: University of Wisconsin Press. 1970. Pp. xv, 227. \$12.50.

Equivalents to the concept of integration had been developed by Eudoxus and Archimedes almost two millennia before differentiation methods, such as that of Fermat, achieved dominance in the calculus of Newton and Leibniz. Integration, as distinct from antidifferentiation, resumed its rightful place in analysis when Cauchy in 1821 and 1823 defined the integral of a continuous function. During the next few decades Dirichlet and Riemann set the stage, well outlined by Hawkins in his first chapter, for the problem central to this volume—the distinction between continuous and integrable functions. The remaining five chapters, covering the years from 1870 to about 1910, describe how, as more pathological types of functions came under scrutiny, theorems had to be amended under new standards of rigor. Questions were raised as to the integrability of functions such as  $\sin[1/\sin(1/x)]$ , which has infinitely many infinitely dense points of discontinuity. We read how, during the 1880s, the role of measure-theoretic properties of sets was examined by such men as Weierstrass and Cantor; and the last decade of the century is described by the author as "a period of transition" in which "the notion of measurability . . . formed the crucial link."



Peano and Jordan, in 1887 and 1892 respectively, had adumbrated the notion of measure of a set, leading in 1895 to Borel's concept of a set of measure zero.

Lebesgue, like Borel a graduate of the École Normale Supérieure, shortly after his graduation published in the *Comptes Rendus* a series of research announcements that culminated in his doctoral thesis of 1902, "Intégrale, longueur, aire," and his classic *Leçons sur intégration* of 1904. These works presented a concept of measure more general than that of Borel and a definition of the integral that included that of Riemann as a special case. In a series of further publications (references are found in Hawkins's ample bibliography) Lebesgue developed his integral into "an analytical tool capable of dealing with—and to a large extent overcoming—the unresolved problems that had arisen in connection with the old theory of integration." His work was warmly received, and its triumph was assured by further abstractions, such as the Lebesgue-Stieltjes integrals of Radon (1913).

Hawkins's volume is not to be digested in a casual perusal by a mathematical tyro; but a reader with seriousness of purpose and a thorough grounding in real analysis will be rewarded with a well-reasoned account of the step-by-step development of one of the most important concepts in modern mathematics.

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CHARLES HARVARD GIBBS-SMITH. *Aviation: An Historical Survey from Its Origins to the End of World War II*. (Science Museum.) London: H. M. Stationery Office; distrib. by British Information Services, New York. 1970. Pp. xvi, 315. \$9.00.

When Charles H. Gibbs-Smith's *The Aeroplane: An Historical Survey* appeared in 1960, it received rave reviews in numerous trade journals. Unfortunately, the significance of the book escaped the academic community, partly because the author so organized his material—less than 150 pages of narrative, followed by numerous quotations, a glossary, charts, and lengthy sections on the development of such things as kites and parachutes—

that the book was more a dictionary or an encyclopedia than a historical survey.

*Aviation: An Historical Survey*, an extensive revision of the former work, corrects some of the organizational problems as well as some of the minor errors. Among other things, the glossary and quotations are kept, but the narrative is expanded (discussions of technical problems are reserved for a later volume). The result is one of the finest books published on the history of aviation, one as impressive for its perspective as for its detail.

Gibbs-Smith devotes almost half his narrative to the pre-Wright brothers era. The long and laborious birth of aviation, he feels, was in many ways more important than the natural maturing in an age of technology. The former involved thinkers and dreamers centuries removed from practical flight, followed by more than a century of "forerunners" who attempted to turn hope into accomplishment. Then, in little more than a decade, success. What followed was mostly one technological development after another.

Gibbs-Smith's sterling reputation, bearing the imprint of an impressive background in aviation history and a sound understanding of the science and technology associated with flying, certifies both his emphasis and his conclusions. His view of the Wright brothers, for example, contrasts sharply with the commonly accepted idea of two lucky bicycle manufacturers. He shows the Wrights to have been first-rate inventors and thorough students of flight who grasped certain essentials that somehow escaped their European counterparts. These essentials, according to Gibbs-Smith, were mastery of glider flight as a necessary prelude to powered flight, acceptance of the best aerodynamic theory, adequate testing before flight, and perhaps most important, the courage born of understanding to build inherent instability into their aircraft. In deliberately making their airplanes so they would have to be piloted with skill, the Wright brothers were selecting the one path that allowed mastery of the air. This, Gibbs-Smith suggests, was the reason why the Wright brothers were flying long distances and turning with ease, while their European rivals were still trying to get airplanes to stay in the air.

The refreshing perspective of *Aviation: An*

*Historical Survey* is, however, only one of its prime qualities. There is also Gibbs-Smith's skillful blending of ideas, budding technology, scientific theory, and the perseverance of pioneers in describing one of the greatest advancements of all time. Hence the book should appeal to the intellectual historian, the historian of science and technology, the aviation buff, and perhaps the area specialist who, thanks to efforts beyond his present horizons, now races with ease to lands and archives that were once well out of reach.

PHILIP M. FLAMMER  
*Air University Review*

BARNET LITVINOFF, editor. *The Letters and Papers of Chaim Weizmann*. Series A, Volume 2, November 1902–August 1903. New York: Oxford University Press, 1971. Pp. xlvii, 489. \$12.75.

This second volume of the letters of the man who was for many years the leading figure in world Zionism maintains the high standard set in the first. One might have some reservations about the wisdom of the decision to publish all the letters in the Weizmann Archives, if only because at the present rate the publication is likely to require several decades. The condensation of letters to Weizmann to extracts or summaries in footnotes is at times awkward, and transliteration from Yiddish and occasionally Russian is not standardized. But overall the editing, translation, and annotation of this multilingual collection are excellent.

If the contents of the letters are relatively unspectacular, it is because in the period covered (November 1902–August 1903) Weizmann was not yet in the forefront of Zionist leadership, and in any case the movement itself was at a low ebb. The present volume closes on the eve of the Sixth Zionist Congress and the great controversy over the so-called Uganda (East Africa) colonization scheme, and instead of Uganda the largest single group of letters deals with Weizmann's activities in behalf of the premature project of establishing a Jewish university in Europe or Palestine. Nevertheless there is much of great interest here. The reader gets glimpses of Weizmann's enormous charm and intelligence and considerable material for Zionist and general Jewish history in the early twentieth century. The highlight of the collec-

tion is undoubtedly Weizmann's long letter of May 6, 1903, to Theodor Herzl, which gives a broad picture of the state of Russian Jewry and especially of its youth—a youth seen as drifting into the arms of socialism and the *Bund* and as alienated by the semiclerical and “Western” official Zionist orientation. Herzl and the early Zionist leadership appear unresponsive to the needs of the East European Jewish masses, whose spokesman Weizmann tried to be.

Future volumes in this monumental series will no doubt be of even greater interest, for they will trace Weizmann's rise and Zionism's stormy history. The high standards set in the early volumes ensure that scholars will find them fascinating to read and, above all, a comprehensive and useful source.

SOLOMON BEINFELD  
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RONALD W. CLARK. *Einstein: The Life and Times*. New York: World Publishing Company, 1971. Pp. xv, 718. \$15.00.

The present state of Einstein studies invites the energetic biographer. There exist vast archival materials largely untouched; a great number of relevant specialized works; and, in the way of competition, recollections of former associates, collections of anecdotes, hagiographies, picture books, potboilers, and “great lives in science.” Mr. Clark, a practiced biographer who only three years ago gave us three generations of Huxleys in one volume, has responded to the invitation with the ambitious *Life and Times* under review.

Mr. Clark's particular contributions are the wide range of his sources and the admirable determination to fashion a full and balanced biography from them. They are, however, less easy to exploit than to discover. Mr. Clark's superficial knowledge of modern physics puts him at a disadvantage. The spirit as well as the content of Einstein's work eludes him; he must substitute romance or mystification for analysis (Einstein “soared up into the mathematical stratosphere where the battle had to be fought”); and he consequently cannot describe his subject's genius in terms of its most characteristic output. Likewise Mr. Clark's innocence of both the history and philosophy of modern science

prevents him from placing Einstein's contributions in context. In particular, he misses the point of the heroic debates between Einstein and Bohr over the interpretation of quantum mechanics, and he exaggerates the bearing of the discussion on the scientific isolation of Einstein's later years. Nor does Mr. Clark appear to have the temperament or style required for his task. Where Einstein is economical, witty, careful, and precise, Clark is repetitious, prolix, pallid, and diffuse.

These characteristics mar even those parts of the book where Clark is most at home. His accounts of Einstein's external career—the employment in the Bern patent office, the steps up the academic ladder, the special professorship in Berlin, the pacifist and Zionist activity, the move to Princeton, the sailing, the fiddling, and so forth—are interesting and informative. But Clark gives his reader little help in constructing a consistent picture of the actor in these events. He emphasizes what he calls “paradoxes”—Einstein the unreligious Zionist, Einstein the pacifist behind the atomic bomb, Einstein the naturalized Swiss and German hater, who clung for twenty years to his position in Berlin. He tells us that the ill and soxless recluse of Princeton, the weary world figure, the uprooted European, differed little from the vigorous and dapper patent clerk of Bern, or from the famous Berlin professor, the friend of Rathenau, Weizmann, and the queen of the Belgians. He describes Einstein's nonscientific views and plans sometimes as “naive,” sometimes as “shrewd,” and sometimes as “prescient.” When one ponders these dicta in the light of Clark's claim that Einstein was “similar in work and outlook” to Max Planck—a strong nationalist, a good German, stiff, formal, and conservative—one conjectures that Mr. Clark has not read Einstein's character any more closely than he has read the theory of relativity.

JOHN L. HEILBRON  
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Berkeley

GLENN B. INFELD. *Disaster at Bari*. New York: Macmillan Company. 1971. Pp. xii, 301. \$7.95.

In August 1943 reports reached the United

States that Germany might use gas. President Roosevelt responded with a public warning and confidential permission for the dispatch of a supply of mustard gas to the Italian theater. Some one hundred tons of the chemical were sent in strictest secrecy to the depot at Bari aboard the *John Harvey*. At this time the Allied air forces were overconfident; Bari, under “that old handmaiden of disaster, multiple command,” lay open, exposed, and well lighted. The Luftwaffe struck on December 2, 1943; it destroyed seventeen ships, damaged eight others, and caused a death toll of over a thousand among Allied military and Italian civilians. The *John Harvey* went down with all its crew and, thus, those who might have given warning were eliminated.

The Bari tragedy was well hushed up at the time and remained rather obscure until now. Infield, himself a pilot in the U.S. Army Air Force during the war, took great pains in gathering the material for his book. In Germany he interviewed General Student and Luftwaffe pilots who served under him; in England officials who had been involved gave information albeit reluctantly; in the U.S. Infield received full cooperation from the military. The bulk of the material, however, came from the author's visit to Bari, his survey of the area, and his discussions with survivors. He has put together a very readable monograph with clever chapter titles and has piled up the evidence to sustain his conclusions. Very few of the 617 military and merchant marine casualties need have died, he states, if knowledge of the chemical agent had been immediately available. “Most of the deaths were due primarily to the prolonged exposure.” Infield insists that “secrecy, then and now, is the main cause of such tragedies. All matters pertaining to chemical warfare agents are cloaked in this ‘iron curtain’ of secrecy, usually to the detriment of all concerned.”

There are no footnotes but the author provides a full bibliography and appendixes, and the sources for specific fact statements are rather clearly indicated by the context of the narrative.

HOWARD M. SMYTH  
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D. C. WATT. *Survey of International Affairs, 1962*. (Issued under the auspices of the Royal Institute of International Affairs.) New York: Oxford University Press. 1970. Pp. ix, 558. \$14.50.

The Royal Institute's superbly useful Survey of International Affairs series has now reached 1962 with the release of the second volume prepared under the editorship of D. C. Watt. In addition to taking overall responsibility for the 1962 volume, Watt has written most of the book himself, delaying publication to take account of "the literary 'fall-out' of the Kennedy régime." This fallout greatly enriches the narrative in a number of chapters, for the *Survey* is based on already published materials and the information now available on the events of 1962 is unusually full, particularly with regard to the involvement of the United States in the world arena.

Given the inherent limitations in writing current history without access to government archives, this volume is admirably broad in coverage, objective in tone, and clear in perspective. Watt summarizes again the Cuban missile crisis and threads his way with clarity through the maze of Anglo-Franco-American policy in Europe, carrying this subject to the defeat of President Kennedy's "Grand Design" at the hands of General de Gaulle early in 1963. Watt includes also a brief treatment of the strategic policies of the nuclear powers (the United States, the Soviet Union, the United Kingdom, and France); a chapter on what he calls the "Intermarium," which deals with Finland, Poland, Yugoslavia, and Austria, with a separate section on the South Tirol; a summary of the crisis in the United Nations; and a very interesting survey of the acquisition of western New Guinea by Indonesia.

John Erickson is responsible for the illuminating discussion of Sino-Soviet relations from October 1961 to January 1963, while John Major treats the difficult problems of Africa, with a special chapter on the explosive relations of France with Algeria. Michael Donelan skillfully compresses into twenty-five pages the financial policies of the Western powers.

The crucial problems of Southeast Asia, the Sino-Indian conflict, and the Arab world centered on Cairo fall to Richard Gott. Gott did not, of course, have available the *Pentagon Papers* when he chronicled the events of 1962 in

Vietnam and Laos, but their revelations probably would not have altered his conclusions materially.

The usefulness of this volume as a reference work is marred by the shabby treatment given by the Oxford editorial staff to senior United States officials below the very top level. Some officials (Rostow and Sorensen) are the victims of mere typographical errors, but the names of others—Arthur M. Schlesinger, Jr., Foy Kohler, Livingston Merchant, and Howard Trivers in the Executive branch and Harry Flood Byrd and Wayne Hays in Congress—seem to have been maltreated through deeper carelessness. The indexer has copied the mistakes of the text and has then added errors of his own. Finally, there is a certain lack of precision in the titles ascribed to various officers, notably Paul Nitze, who turns up in the wrong department. Americans surely deserve editorial care equal to that bestowed on statesmen from Europe, Asia, and Africa.

RICHARDSON DOUGALL  
*Department of State*

## ANCIENT

HANS GOEDICKE. *The Report about the Dispute of a Man with His Ba*. (Baltimore: Johns Hopkins Press. 1970. Pp. vii, 248. \$12.00.

The ancient Egyptian literary work commonly known by the title of the *Lebensmüde* and here given a new title is one of the best known of all ancient works of literature, and many Egyptologists have made attempts to translate it and to understand its complexities. This new edition is an important contribution to the discussion that has continued ever since the first edition by Erman appeared in 1896, and the full treatment of both subject matter and language in this study advances our understanding of a fascinating document.

The text almost certainly dates from the first Intermediate Period or early in the Middle Kingdom and is one example of a group of writings all of which have, so far as we can understand them, considerable literary merit and are known for their pessimistic view of life and of the condition of Egypt at the time. The work studied here does not entirely fall into this category and as the editor shows is a specu-

lative work concerning the nature of the life of man. It takes the form of a dialogue dispute between a man and his *ba*, the man defending an idealistic view of the afterlife while his *ba* takes a more practical and material view and deprecates the excessive concern of the man with death and its aftermath. The nature of the *ba*, that important element in Egyptian religious belief sometimes translated as "soul," is discussed by the editor at some length.

The book consists of a very interesting and elaborate discussion of the nature of the text, followed by a detailed and scholarly commentary on grammatical, lexical, and semantic aspects. The hieratic text is given by photographs of the original papyrus accompanied by a hieroglyphic transcription. This is preceded by a straightforward English translation from which it can be seen how difficult it is, in view of our limited knowledge of Egyptian, to have a proper understanding of religious and philosophical writings.

The non-Egyptological reader may well find it difficult to appreciate how the very complex and closely argued thesis in the main discussion can be sustained by our still hazy knowledge of what the document really says. The edition and commentary is a tour de force and gives some glimmering of the nature of Egyptian thought and literature.

P. L. SHINNIE  
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E. W. MARSDEN. *Greek and Roman Artillery: Technical Treatises*. New York: Oxford University Press. 1971. Pp. xviii, 277, 14 plates. \$16.00.

Engineering handbooks are a byword for meager style and turbid jargon; without the accompanying diagrams they would be well-nigh impenetrable. Ancient technologists conform to this pattern except that their illustrations, when not totally obliterated, are corrupt. Recently one cluster of these perplexing writers has engaged the attention of E. W. Marsden of Liverpool; his *Greek and Roman Artillery: Historical Development* (1969) collated the evidence in a novel but plausible synthesis. Syracuse, it seems, gave us the crossbow in 399 B.C. Somewhat later, perhaps about 350 B.C., Philip's Macedonian artificers invented the cata-

pult, which despite appearances is not a crossbow—the arms are not joined but are mounted individually in twisted rope springs. Sundry refinements followed; certain larger versions were adapted for stones. Altogether the catapult reigned supreme without radical innovation until the advent of the trebuchet in the twelfth century.

Now we have the companion volume with texts of the five primary sources, translations (often the first), commentary, and drawings. The first primary source, Ctesibius of Alexandria's *Artillery* as re-edited by Hero of Alexandria, discusses the crossbow and modifications of the catapult until 270 B.C. One quibble: the text figures for euthytone catapults show the curved side of the crossbeams facing backward; according to a sketch transmitted in the manuscript they should be reversed. The second source, Bito of Pergamum's *How to make War Engines and Artillery*, specifically deals with four crossbows, a siege tower, and a scaling ladder. Marsden's date (ca. 240 B.C.) makes Bito old-fashioned; with the alternative, 140, he is hopelessly anachronistic. "The most curious thing about this work is that no one so far has been able to understand it," A. G. Drachmann commented in 1963. Marsden unravels every knot. Philo of Byzantium's *Artillery*, the third source, is a discussion of standard catapults for stones and javelins and four experimental designs. It was probably written between 230 and 200 B.C., for it ignores developments after 200 B.C. Marsden has made a full-size three-span arrow firer, weighing seventy-five pounds and shooting over three hundred yards. Vitruvius's *Architecture* (10. 10–12. [ca. 25 B.C.]), the fourth source, describes two catapults. The figures in his table of dimensions for different calibers are unexpectedly low; Marsden suggests that they are Roman inches rather than Greek digits. The fifth source, Hero of Alexandria's *The Hand-Catapult* (after A.D. 62), has baffled commentators such as Rudolf Schneider ("das Bruchstück eines . . . Lexikons für Konstrukteure," "vom Geschützwesen kaum eine Spur" [*Römische Mitteilungen* (1906)]). In Marsden's view it sets forth the standard Imperial arrow thrower that was pictured on Trajan's column and mentioned by fourth-century authors. Paradoxically some sources designate one component as the "bow"; it is not a bowstave, says



Marsden, but the sighting arch. His reconstructed *cheiroballesta* shoots 150 yards.

The final chapter concerns the sturdy "one arm" or "wild ass" (medieval mangonel), Hellenistic invention that finds no description before Ammianus. Marsden's scale model shot small missiles almost five hundred yards.

In short Marsden has curbed a whole stable of uninviting and refractory texts. His book is serviceable, ingenious, and persuasive.

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C. BRADFORD WELLES. *Alexander and the Hellenistic World*. Toronto: A. M. Hakkert. 1970. Pp. 265. Cloth \$8.00, paper \$2.95.

This book was ready for the publishers at the time of the author's sudden death in 1969, and it was seen through the press by Alan Samuel, who has added a brief epilogue. Welles inherited from Rostovtzeff the tradition of introducing Yale undergraduates to Hellenistic history, and every teacher who has attempted a similar task has wanted a book that complemented Tarn's *Hellenistic Civilisation* by offering an account of political history with more detail than in his introductory chapter. The difficulty of writing such a book is enormous, even for someone who is as perfect a master of his subject as Welles was.

After a brief introduction describing the achievement of Philip, there is an account of Alexander in chapter 2 in the "modern" manner, in full reaction against the interpretation of Tarn, who defended Alexander against his attackers and believed he was influenced in great part by idealistic motives. Welles insists on Alexander's ruthlessness, invites us to believe that his detractors are less likely to be lying than his defenders, and that Arrian's faith in the veracity of Ptolemy and Aristobulus is unjustified. The argument is presented with skill and conviction, but to those of us who want to know why Alexander was respected by some while others feared and hated him, it may seem rather one-sided.

The next chapter sets out to tell the story of the Hellenistic kingdoms from the death of Alexander to the battle of Actium, covering nearly three hundred years in a hundred pages.

It is perhaps too much to expect that a summary narrative of this kind will guide a student safely through the forest of these troubled centuries. It is easy to say that Welles included too much detail, but if one starts to cut down trees in the hope of better revealing the character of the forest, the result may be to transform the forest into a desert that has neither character nor form. Is the traditional "survey" the best method of introducing students to this period of history? It seems to me a pity that Welles adopted the plan of a single continuous narrative, attempting to keep track of simultaneous events in several different areas, instead of splitting up the narrative with digressions or giving separate accounts of the individual dynasties and the different problems and themes that call for discussion. Must an account of political developments necessarily precede a discussion of society and culture, and must the story necessarily be told in chronological order? Would it be helpful to describe the situation as it was about 250 B.C. before trying to explain how this result was reached?

I raise these questions the more readily because the highest praise is due to the subsequent chapters on "Social and Economic Aspects" and "Hellenistic Culture." Inevitably every reader will miss something that he would have liked to see included (my own regret was to find so little attention devoted to Hellenistic schools and the development of the gymnasium), but this is where a student will discover how much is known about the Hellenistic world that cannot be known about earlier periods of ancient history and how much knowledge has been gleaned from papyri and inscriptions.

LIONEL PEARSON  
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STYLIANOS SPYRIDAKIS. *Ptolemaic Itanos and Hellenistic Crete*. (University of California Publications in History, Volume 82.) Berkeley and Los Angeles: University of California Press. 1970. Pp. 113. \$3.00.

The title of this small volume is somewhat misleading; much of the text deals neither with Hellenistic Crete nor Ptolemaic Itanos but with eastern Crete in the Hellenistic period. In brief, Spyridakis argues that by Hellenistic times three cities of consequence, Itanos, Prai-



sos, and Hierapytna, survived in this area. Because of the "natural antagonism" between Eteocretan and Dorian, an explosive atmosphere, rife with tension and continuous conflict, prevailed. This incessant warfare, he believes, prompted the Itanians to invite the Ptolemies into their city about 270 B.C. Once established there the Ptolemies promoted peace in the area and used Itanos as a base for recruiting mercenaries until their deteriorating position in the eastern Aegean forced them to withdraw about 200 B.C., though they did return briefly under Ptolemy Philometor.

While Spyridakis's discussions of individual inscriptions and particular events are adequate, the work suffers from a number of serious defects. Only a few can be mentioned here. His desire to include everything, relevant or not, has resulted in a poorly organized volume. Matters of consequence are ill defined; thus on a single page (p. 3), Itanos is referred to as a virtual protectorate, a protectorate, and a possession of the Ptolemies, and the confusion is never entirely resolved (cf. pp. 82, 88). Of greater consequence, the author's conclusions seem to emerge more from his own basic assumptions than from the evidence he presents. His discussion of events after 145 B.C. demonstrates conclusively that warfare did not decline in eastern Crete after the destruction of Praisos, the one supposedly Eteocretan stronghold in the area. This negates his conclusion that warfare between Itanos, Praisos, and Hierapytna derived from some natural antagonism between Eteocretan and Dorian.

Anyone interested in Hellenistic Crete will find Van Effenterre more rewarding; anyone interested in Ptolemaic Itanos would be well advised to draw his own conclusions from the epigraphical evidence assembled by Guarducci.

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JAMES H. OLIVER. *Marcus Aurelius: Aspects of Civic and Cultural Policy in the East*. (Hesperia: Supplement XIII.) Princeton: American School of Classical Studies at Athens. 1970. Pp. xv, 160, 8 plates. \$10.00.

The purpose of this work is to publish a text, translation, and discussion of a remarkable document of Marcus Aurelius from Athens, con-

taining his decisions in cases concerning office holding, membership of the council and Areopagus, and the appointment of the Athenian members of the Panhellenion (the Pan-Hellenic council of cities instituted by Hadrian). Documents revealing decisions by the Roman emperors continue to be published in ever-increasing numbers. But this one surpasses in importance and difficulty both the *Tabula Banasitana*, also from Marcus Aurelius' reign, now being published by William Seston in *Comptes-rendus de l'Académie des Inscriptions*, and the long series of imperial letters from the excavations of Aphrodisias.

The puzzles it presents as a document are rather briefly touched on by Oliver (pp. 35-37), as also by C. P. Jones in his prompt and valuable re-edition of the text, "A New Letter of Marcus Aurelius to the Athenians" (*Zeitschrift für Papyrologie und Epigraphik* 8 [1971]: 161). In fact it is clearly not a letter but appears from its final paragraph to be a collection of imperial legal decisions on Athenian cases put together and translated into Greek at the imperial court. The occasion and purpose of the compilation is not clear from internal evidence but may have been mentioned in the missing prescript.

Oliver is similarly brief on the background of imperial jurisdiction, a major, indeed central, part of the emperor's functions, on which the new document provides a flood of new evidence and new problems. Instead, as his wide-ranging title implies, he moves immediately to a series of problems about the constitution of Athens, events in the reign of Marcus Aurelius, and, above all, the Panhellenion, on which his chapter 4 provides the fullest collection of evidence now available. In short, he has published, with admirable speed, a vast and fascinating new document and has also provided with it both less and more than the reader could have hoped for.

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JÜRGEN DEININGER. *Der politische Widerstand gegen Rom in Griechenland, 217-86 v. Chr.* Berlin: Walter de Gruyter. 1971. Pp. xviii, 279. DM 48.

The title of this book is to be taken with strict

literalness; it is not a discussion of Roman imperialism in Greece but of the resistance, mainly the internal resistance, of Greek states to the expansion of Roman political and military power in their country. Roman imperialism is taken as an unanalyzed fact. The foregoing is a favorable criticism; it is good and serviceable to have a book of this kind. Nevertheless it is perhaps not quite so original as the author thinks. Writers in the past held the concept that the "democrats" were anti-Roman and the "oligarchs" pro-Roman; Deininger points out that we have been aware for some decades that the word "democracy" in the Hellenistic Age regularly connotes what classical Greece had known by "oligarchy." Therefore, he argues instead that the upper classes were divided into pro- and anti-Roman factions while *hoi polloi* were regularly and consistently anti-Roman, whether moderately or extremely. All of this is quite true, but one doubts whether in recent decades even those scholars who sometimes slipped, anachronistically, into using democratic in its classical sense really understood the word in that sense. One notes that Deininger himself uses the word anachronistically (pp. 240-41).

Deininger, in effect, discerns three phases in Greek anti-Romanism; the first, limited to the late third century, stigmatized the Romans as barbarians. During the second phase, from the Second Macedonian (or better, the Aetolian) War to the Third Macedonian War, there was a quarrel between anti- and pro-Roman upper-class leaders, the former supported by *hoi polloi*. The defeat of Macedonia and the rigorous suppression of anti-Romanism in Greece in 168-67 B.C. ended upper-class resistance to Rome; yet Deininger must admit that Greek leaders during the Achaean War, two decades later, certainly included upper-class figures (cf. pp. 228, 234). Finally, one may quibble about the terminal date as marking the absolute end of political resistance to Rome: even in the principate, Athens, at least, witnessed such resistance, if anti-Roman riots of *hoi polloi* meant anything.

In all, this is a useful book but not as important or profound as is the author's previously published study of Roman provincial councils.

STEWART IRVIN OOST  
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CHESTER G. STARR. *The Ancient Romans*. New York: Oxford University Press, 1971. Pp. 256. \$7.95.

It is occasion for praise and gratitude when a distinguished scholar sets his hand to an introductory work aimed at high school and junior college audiences. Students who enter Roman history via Professor Starr's admirable volume will not have to divest themselves of multiple misconceptions when they reach more advanced levels.

The work is intelligently conceived and successfully executed. Starr abandons the traditional textbook's concern for "coverage." There is not much bald narrative or tedious recital of facts. Four major subjects receive emphasis: the clash of Rome and Carthage, the career of Julius Caesar, the Antonine Age, and the rise of Christianity. Starr rightly prefers ample treatment of a few topics to a superficial touching of all bases. Special essays are devoted to certain other items, like the Etruscans, the army, Roman law, and late imperial art; Starr offers them as separate objects of attention, not buried in a mass of narrative. Excellent illustrations, charts, and maps grace the text; and the illustrations possess full explanatory comments, not just standard captions. Selections from the sources are placed at the end of each major section, organized under headings and elucidated by brief introductions. A convenient and appropriate design.

Some criticisms may be registered. It is misleading to portray the Twelve Tables as guaranteeing fair justice to all citizens (pp. 15-16) and quite erroneous to apply the term "democracy" to the Roman Republic (p. 16). Montesquieu and the Founding Fathers may have seen in the Republican constitution a system of checks and balances and a separation of powers, but their notions ought not to be confused with the facts (p. 59). The equestrian order as a "separate wealthy class" can hardly be discerned in the third century B.C. (p. 25). Nor should the equestrians be identified with commercial and financial leaders (p. 70). Starr omits all mention of agrarian problems and barely notices the lower classes in the late Republic (pp. 71ff). The concept of a "golden age of the good emperors" (p. 180) is belied by many of the sources that he himself includes. Little attention is paid to the Jewish back-

ground of Christianity and no hint given of the conflict in early Christianity over conversion of the Gentiles (pp. 185–87). The stress on imperial opposition and persecution of Christians fails to make clear how little official persecution there was before the mid-third century A.D. (pp. 189–92).

It is easy to find fault. But the merits of Starr's work earn appreciation. He has achieved lucidity without excessive oversimplification. The problems confronting an ancient historian and the tentativeness of conclusions are emphasized—a feature uncommon in introductory books. As an initial exposure to Roman history for younger students this volume has few peers.

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DONALD R. DUDLEY. *The Romans: 850 B.C.–A.D. 337*. (The History of Human Society.) New York: Alfred A. Knopf, 1970. Pp. xxvi, 316. \$7.95.

This volume in the History of Human Society series has an excellent introductory essay by the editor, J. H. Plumb. At one point Plumb reveals the assignment of the individual authors in the series: "Their aim will be to reconstruct the societies on which they are experts. They will lay bare the structure of their societies—their economic basis, their social organizations, their aspirations, their cultures, their religions, and their conflicts. At the same time they will give a sense of what it was like to have lived in them."

Dudley obviously wrote this book with the general series in mind, but the task is difficult for a Roman historian. The sources for Roman history, particularly the Roman Republic, are essentially political. The ancient Roman historians did not write about "human society," and Dudley knows his Roman historians well, perhaps too well to write the kind of book this series calls for.

Dudley's problem is evident in his treatment of the Republic. The social and economic problems of the Republic are directly related to politics and warfare, and the historian who writes about those problems must either assume that his readers know the political background

or he must summarize it for them. Since the History of Human Society series is not directed to specialists, Dudley decided to summarize. Unfortunately he did not strike a balance between political background and the problems of society at large. There are too many brief chapters on politics. (The chapter on the three Carthaginian wars is five pages long, and the one on Antony, Cleopatra, and Octavian is not quite three full pages.) Marius is introduced as the creator of the professional army, but the reader is not told what happened to him after his victory over the Cimbri and Teutones.

The discussion of the Roman Empire is much more in keeping with the purpose of the series. There is more evidence, particularly archaeological, for the history of society in the Roman Empire, and Dudley makes good use of it. There are brief chapters on such topics as "Travel," "Public Careers," "Frontiers," and "Universal Religions." In addition there are two good chapters on the provinces. Although Dudley is probably not critical enough of Christian tradition, his judgment is generally good and his treatment interesting.

Unfortunately almost half of the book is devoted to the Roman Republic and too much of that to unsatisfactory political summary. There are points in the last half of the book where the reader will begin to understand "what it was like to have lived" under Roman rule, but in general the book fails to give a clear picture of Roman society and culture.

There is a good index and bibliography. In the acknowledgements the author says that the illustrations were planned as "an integral part of the book," but some of them are unnecessarily poor, especially the ones of Hadrian's Wall, Masada, and Cosa. The title of the book is somewhat misleading since it does not end with the death of Constantine, as the date suggests, but with the final collapse of the Roman Empire in the West.

ARTHER FERRILL  
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ROBERT E. A. PALMER. *The Archaic Community of the Romans*. New York: Cambridge University Press, 1970. Pp. ix, 328. \$12.50.

Each citizen of republican Rome found himself in the peculiar position of belonging to three

different groups and three different assemblies involved in the functions of government: he was a member of a *curia* (*comitia curiata*), a century (*comitia centuriata*), and a tribe (*comitia tributa*). Of these the oldest and the most important for our knowledge of archaic Rome is the *curia*. Attempting to see through the haze of myth in Livy, Dionysius, and Varro by critical sifting of the evidence and with the help of heortological records, Palmer has given us his interpretation of what the *curiae* were and what role they played in the early government. At times he has let his imagination run too far and has seen *curiae* or suspected their influence in inscriptions where, in fact, they are not mentioned—for example, the Caere-tane *curia* is really attested (*CIL* XI, 3593), but the *curator* of *CIL* XI, 3614 is a *curator civitatis*, and there is nothing there to suggest any reference to a *curia*. In general, however, Palmer's work contains much sound reasoning. He is not a slave to traditional scholarly views and is quite ready to break new ground. But his excavations will not always support the weight of his superstructure.

Palmer believes that there were, as tradition says, thirty *curiae*, but that the last three of them were not formed until the very early years of the Republic. He accepts a common etymology for the word (*co-vir-ya*) and defines it as an "assemblage or congregation cooperating by common consent." The Quirites turn out to be the members of the *curiae*. He concludes that the *curiae* were originally the distinct ethnic groups that were brought together in the hill district of Rome, eventually forming the Roman state. The assembly of all the *curiae* largely gave direction or advice to that united state, but it in turn was presided over and controlled by that very king whom it put into power. This curiate government was conservative and was ultimately confronted by a reactionary and progressive movement called the centuriate or Servian constitution. The opposition of the curiate to the centuriate can be seen, Palmer believes, in the power of the curiate assembly to elect the plebeian tribunes and the military tribunes, power the curiate ultimately lost. The new military centuriate system curbed and limited curial religion, eventually winning political success, but down to the Licinian-Sextian Laws of 367 B.C. the two sys-

tems existed side by side. Thus, for almost 150 years these two systems coexisted, the curiate system gradually losing power in all but a few areas. Apart from an occasional vagueness and a certain inability to see the weakness of details that arise from his main thesis, Palmer's interpretation of the curiate system makes good sense. For one thing it illuminates the conditions under which the curiate assembly had acquired the power to confer imperium on the kings and, later, on the higher magistrates.

This is a scholarly work, not easy to read, certain to evoke controversy, and required reading for all those interested in early Rome. But the reader must be careful not to confuse Palmer's theories with established truth.

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STEPHEN BENKO and JOHN J. O'ROURKE [editors]. *The Catacombs and the Colosseum: The Roman Empire as the Setting of Primitive Christianity*. Valley Forge, Pa.: Judson Press. 1971. Pp. 318. \$6.95.

When he composed his *History of the Decline and Fall of the Roman Empire*, Edward Gibbon chose to introduce the Christian Church to his readers only after discussing Diocletian's tetrarchy. After two centuries of scholarly endeavor the Gibbonian approach is still widespread. Today a historian of the Roman Empire is likely to postpone an assessment of the Christian Church until the eve of its fateful alliance with the Roman state. The dangers of this procedure are manifest: despite precautions the Roman historian inevitably glosses over some aspects of the early Church; and just as inevitably his reader fails to understand why the Church was so powerful in late antiquity.

The present volume is a work of church history. Its purpose is to call attention to an analogous sin of church historians, their failure to take full account of the Roman Empire in their study of the early Church. This work consists of a series of essays, most of which were presented at the Philadelphia Seminar on Christian Origins in 1969, on the Roman environment of the Church to A.D. 135. Following an introduction by Robert M. Grant, there are treatises on early imperial politics by Stephen

Benko; the institutions and social structure of the principate by John G. Gager, Clarence L. Lee, John T. Townsend, and John J. O'Rourke; administration in the early Empire by James L. Jones and William White, Jr.; and early imperial religion by Robert A. Kraft, Donald Winslow, Gerhard Krodel, and Robert L. Wilken.

Since this work is intended mostly for the student of the early Church, church historians will be the primary judges of its timeliness and value. But the essays will also be of use to the historian of the Roman Empire, and it is this aspect of the book that I wish to emphasize. The historical merit of these essays is not immediately apparent, for at first glance they seem to offer simply a conventional synthesis of the Julio-Claudian, Flavian, and early Antonine principate. Although some essays—notably those of Gager, Jones, and Wilken—are excellent, no new discoveries are presented. Furthermore, there are numerous small errors of fact and interpretation. In Grant's introduction, for example, Hadrian's favorite Antinous becomes "Antoninus" (p. 18). Grant and Kraft (pp. 17 and 82), influenced by the recently discovered inscription designating Pontius Pilate as [*praef*]ectus Iuda[ea]e, ignore the fact that after the accession of Claudius provincial governors of equestrian rank generally bore the title of *procurator* (see A. H. M. Jones, *Studies in Roman Government and Law* [Oxford, 1960], 115–25). The Roman historian, then, might view this collection of essays solely as an orthodox piece of early imperial history, but in doing so he would miss a spirit of curiosity that is evident throughout the book—a spirit that the contributors obviously wish to be contagious.

In effect, the results of this work are of less importance than the interest behind them. A group of church historians has compiled some fairly up-to-date information about the Roman environment of the early Church. Might Roman historians conduct a similar investigation of Judaism and Christianity during the early principate? All contributors to the present volume implicitly or explicitly invite a response from the historian. Grant's explicit invitation (p. 24) may serve as a Parthian shot at the Gibbonian approach: "The early history of Christianity is Roman history, and I should

claim that Roman history itself needs the collaboration of those who try to relate the Christian movement to the whole life of the Empire."

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EDITH MARY WIGHTMAN. *Roman Trier and the Treveri*. New York: Praeger Publishers. 1970. Pp. 320. \$15.00.

This study is intended as a survey of the historical development of the land of the Treveri under the Roman Empire. The work is divided into seven chapters on the land and the people, the Treveri and Rome, the city of Trier, the countryside, daily life and economics, and religion. Also included is a four-page epilogue on the transition to the Middle Ages, a forty-five page bibliography, twenty-four plates, twenty-five figures, and eight maps. Despite its scholarly appearance the book is intended essentially for the "reader with a general interest in Roman studies." Experts are encouraged to read between the lines because the work is not adequately documented. The use of Latin quotations and the frequent allusions to scholarly controversies, however, seem to project this study beyond the competence of all but the most dedicated amateur.

These drawbacks might discourage the reader of even a great synthesis, but here they are a blessing in disguise since they will keep both experts and amateurs from wasting their time. Chronology, an essential element for the study of both history and development, is largely ignored. It might be charitable to suggest that the archeological record is insufficiently complete at this time to provide the exact chronology necessary for writing history. In economics, the one area where the author seems to have some notion of development, she misses the significance of vital evidence. She maintains, largely on the basis of mosaic evidence, that the Trier area reached its peak in prosperity during the late second and early third centuries. Yet, during the later second century cheap sandstone replaced the more expensive and more durable limestone as the common building material.

Perhaps it is unfair to apply the criteria of



historical criticism to this work. An author whose speculations lead her to attribute causality to "the extravagant Celtic temperament" seems to lack a professional grasp of historical method. This book seems to be an archeological catalog comprised largely of descriptions of artifacts. The section on villas reads like a real estate salesman's brochure. The author's justification for discussing an artifact is frequently its "most interesting" construction or "very interesting" workmanship rather than its value as evidence for historical development.

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BARRY CUNLIFFE. *Fishbourne: A Roman Palace and Its Garden*. Baltimore: Johns Hopkins Press, 1971. Pp. 228. \$15.00.

Readers of the *Journal of Roman Studies* will recognize the fine ability of British archeologists to reconstruct the Roman presence in Britain from scanty evidence. When the physical remains are abundant, as on the vast site at Fishbourne near Chichester in Sussex, and are meticulously excavated, the reality of Roman Britain may be dramatically restored. Barry Cunliffe, who excavated Fishbourne, 1961-69, and supervised its restoration, has provided the visitor with the experience of a Roman palace and garden, sensitively preserved in a suburban setting largely with the help of volunteers and the local community. Although he has published a lengthy scientific record of the excavations elsewhere (1971), Cunliffe's book, addressed to a broad public, is a model of *haute vulgarisation*, clearly and imaginatively written, well illustrated with good photographs, plans, and reconstructions, and neatly demonstrative of archeological methods and Roman architecture.

The excavator's concern for the topographical implications of the site, its changing occupants, and the architectural history of the palace articulate his skillful presentation. The geographic nature of the site on the edge of a marshy seashore limited activity in pre-Roman times to salt-panning and fishing, jeopardized and complicated building during the Roman occupation, and probably contributed to the abandonment of the place in the fourth cen-

tury. Fishbourne's location on the south coast of England opened it to Roman conquest from Gaul, then made the area important as a secure base for Roman operations in central Britain, and last exposed the place to the barbarian raids that ravaged the coastline. A similar interest in the occupants of the site enlivens Cunliffe's descriptions of the palace during its 250 years of settled inhabitation and culminates in a fine bit of historical detection that offers Tiberius Claudius Cogidubnus, client-king of the area under Claudius and the Flavians, as the probable owner and patron of the palace and gardens at Fishbourne.

It is the palace itself that captures our attention, the largest (about 450 by 500 feet), the costliest (approximately \$3,500,000), and the most lavishly appointed yet discovered in Roman Britain. Although the site was first occupied by Claudian military buildings, later transformed into a proto palace under Nero, the vast palace seems to have been deliberately planned and built in the Flavian period by an architect who adapted Italian models and building practices to local materials, crafts, and climatic conditions. If Cunliffe's account of the adaptive process is fascinating, so, too, is his careful description of the design, construction, and decoration of the palace, of its division into public, administrative, and residential wings, and of the magnificent formal garden (250 by 330 feet), fully enclosed by the architecture that it complemented.

Frequently repaired and remodeled in the second and third centuries, the palace at Fishbourne was destroyed by a great fire ca. 300 A.D., and its building materials were subsequently looted or broken by plowing. But the palace and the life it contained have been vividly reclaimed by the author in his conscientious and entertaining account.

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JOHN HOLLAND SMITH. *Constantine the Great*. New York: Charles Scribner's Sons, 1971. Pp. 359. \$8.95.

Constantine the Great has more than one thing in common with the other figure from the ancient world who has merited the same epithet: Alexander III of Macedon. Both started a new



age. Both had an *instinctus divinitatis* about them, although its manifestations were different. Both changed the course of history about as much as individual men can hope to do. And each generation of scholars has found a new Constantine and Alexander the Great. The nineteenth century stripped Constantine of his Christianity and left him a hard-boiled politician. This century has allowed him to be at least sincerely superstitious, a child of his age.

John Holland Smith's Constantine is a compromise figure. He resurrects the view that Constantine's vision before the battle of the Milvian Bridge, if it was anything at all, was the sun high in the atmosphere shining through a veil of ice crystals. Visions in the late Empire are easier to explain by psychology. Like many before him, Smith sees something significant in the fact that Constantine waited until near death before he received baptism. Yet deathbed baptism was a common practice of the time; even Theodosius the Great planned to receive his baptism then, although he miscalculated and recovered from what he thought was his last illness after baptism. There is no point resurrecting this old chestnut.

At the same time, something that "may or may not have been sudden conversion to faith in Christ" happened before the battle of the Milvian Bridge. Smith does not commit himself. There is nothing wrong with suspending judgment, but Smith's compromise Constantine is not quite believable.

Smith generally writes from original sources with disappointingly few references to modern scholarship. There are occasional errors in judgment: he argues, for instance, that Constantine's edicts of 326 concerning sexual irregularities could not refer to the "affair" of Crispus and Fausta, since they do not mention it specifically. However, Smith's *Constantine* is a readable biography, no great contribution to scholarship, but reasonably reliable.

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## MEDIEVAL

EDWARD PETERS. *The Shadow King: Rex Inutilis in Medieval Law and Literature, 751-1327*. New

Haven: Yale University Press. 1970. Pp. xv, 257. \$10.00.

O, what pity is it/ That he had not so trimmed and dressed this land/ As we this garden! (*Richard II*, III, iv.) Medieval men did not normally think of kings as gardeners. The problem that concerned Shakespeare in *Richard II*, however, was one that had long concerned them as well: the king who was unsuitable to his high dignity. In this volume Mr. Peters has assembled some aspects of the history *rex inutilis* from the deposition of Childeric III to the deposition of Edward II. He has cast his net widely. Childeric, Louis the Pious, Boso of Vienne, Arnulf of Corinthia, and Pepin of Aquitaine—as well as Gregory VII's use of this early deposition tradition—have been caught in the first chapter; Beowulf, the *chansons de geste*, the *Heimskringla*, and the early Arthurian romances in the second; and a hundred years of canonist discussions in the third. From these he moves to the deposition of Sancho II of Portugal by Innocent IV in 1245, later Arthurian literature, the abdication of Celestine V, and, finally, the depositions of Adolf of Nassau and Edward II.

Mr. Peters is at his best when analyzing particular texts. His discussion of the Childeric reference in Gregory VII's letter to bishop Hermann of Metz is one of several that are both perceptive and persuasive. Medieval historians will also be grateful for the extended treatment of Sancho's deposition, an event that has not won the attention it clearly deserves in histories of *regnum* and *sacerdotium* in the Middle Ages.

The collection of so many events and literary traditions between the covers of a book ought to be justified by a synthetic vision that gives them patterned relationships and significance. The present book, however, does not do so. It is a collection of studies on a theme rather than a coherent history. The result is, in places, a curious imbalance, in others a piece without a place. In the chapter on the Arthurian romances, for example, he gives us an elaborate summary of the plots of the Vulgate narratives; in the chapter on canonist deposition theory, however, only the briefest mention is made of legal discussions concerning episcopal *inutilitas*, though this discussion bears directly on the deposition of King Sancho II. In

another chapter there is a lengthy and very interesting discussion of changing theological conceptions of *acedia*. But this is connected with the theme of the chapter in only the vaguest possible way ("the concepts of *acedia* and other vices . . . had contributed to a background against which royal inadequacy might be viewed") and then drops completely from sight. If Mr. Peters is really saying what the structure of his book implies—that the phenomena he discusses are parallel but not connected—it would have been interesting to have his reflections on how the legal, the literary, and the moral-philosophical traditions managed to go their separate ways for so long within a single intellectual culture. As it stands, the book will serve as a collection of well-crafted articles.

FREDRIC CHEYETTE  
Amherst College

IRVING A. AGUS. *The Heroic Age of Franco-German Jewry: The Jews of Germany and France of the Tenth and Eleventh Centuries, Pioneers and Builders of Town-Life, Town-Government and Institutions*. New York: Yeshiva University Press; distrib. by Bloch Publishing Company, New York. 1969. Pp. xi, 380. \$12.00.

There is no question that Professor Agus knows his *Responsa* literature and has a superb understanding of Jewish intramural relations. How can one properly discuss the Jews as "pioneers and builders of town-life," however, when one not only does not discuss the town but also makes no use of recent socioeconomic scholarship that deals with the problems of the period's urban and economic development? Professor Agus's other studies based upon the *Responsa* are the chief authorities for this work: in chapter 1, for example, 60 of 131 footnotes are from others of Agus's works; in chapter 4, the proportion is 150 of 178; in chapter 7, it is 59 of 62, while in the crucial last chapter (11, "Relationship between Jews and Non-Jews") it is 56 of 65 footnotes.

Because Agus pays scant attention to non-Jewish material, he overstates his position. He speaks of "The Heroic Age," and the "pioneers," yet I found neither heroes nor pioneers in this work. While it is true that Jews did make the dangerous trip to the East (as did

some Christian pilgrims), most of the Jews of whom he writes lived in the established communities west of the Rhine. Mainz seems to be the eastern outpost in Germany. The author states that "the Jews were the first self-ruling town dwellers of Western Europe in the early Middle Ages. In many places northwest of the Alps where a town sprung up, it thus developed around a nucleus of a group of Jewish families organized as a community." Not only is no evidence cited for this, but when Agus does discuss Jewish resettlement it is in terms of being invited into an established community.

The author briefly quotes August Kluckholm (1857) and James Westfall Thompson (1928) as "modern scholars" describing life in the tenth and eleventh centuries. Life during that period was indeed difficult, but to conclude that Europe was "war-torn, constantly feuding, robber infested, and completely lawless" is an overstatement that modern (later than 1928) scholarship would not support.

The late Professor Cecil Roth once warned against neglecting the "hidden Jews," that is, the poor Jew out in the countryside. Professor Agus denies their existence. For him the source of the Franco-German Jew (and for the entire East European Jewry) was about 5,000 (or 1,000 families) rich, prosperous, landowning merchants with a "tremendous dedication to the study and practice of Judaism." Nowhere is this proven.

There is another major problem. The book is impersonal and general. The reader is constantly faced by a lack of specific names, dates, and places: "The Queen of Hungary relied on two Jews. . . . She often sent one of them on important missions." Which queen? When? Where? The footnote in this case cites Agus's two-volume *Urban Civilization* (p. 232). The reader learns that Jew A did this, that, or the other; but where or when is not stated. It would seem as though time and place had no meaning for Jewish life.

There are some minor problems: what, for example, is R. Glaber's first name? There is a choice: Rudolfus, Ralph, or Raoul. Footnote 1 of chapter 3 (p. 96) starts out "See *ibid.*, pp. 99–100, where," and ends, "See *ibid.*, p. 101, note C." I presume the *ibid.* refers to his two-

volume *Urban Civilization*, cited in footnote 107 (p. 77) of chapter 2.

Agus's work is excellent as a study of Jewish life, and it is also helpful as a supplement for European economic development (the sections on the Jew as a borrower of Christian capital, and on the purchasing power of a pound of silver are significant), but when Agus writes of the non-Jewish world or when he begins to compare and contrast, let the reader beware.

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WOLFGANG VON STROMER. *Oberdeutsche Hochfinanz, 1350-1450*. In three volumes. (*Vierteljahrschrift für Sozial- und Wirtschaftsgeschichte*, Beihefte, Numbers 55-57.) Wiesbaden: Franz Steiner Verlag GmbH. 1970. Pp. xxi, 218; 220-460; 463-608. DM 30; DM 30; DM 36.

URSULA SCHWARZKOPF. *Die Rechnungslegung des Humbert de Plaine über die Jahre 1448 bis 1452: Eine Studie zur Amtsführung des burgundischen Maître de la Chambre aux Deniers*. (Veröffentlichungen des Max-Planck-Instituts für Geschichte, Number 23.) Göttingen: Vandenhoeck & Ruprecht. 1970. Pp. 217. DM 26.

These three numbers of the *Vierteiljahrschrift für Sozial- und Wirtschaftsgeschichte* are designed to be bound together as a single volume of some six hundred pages. The first traces the outlines of a general commercial-financial history centering on the city of Nuremberg from the mid-fourteenth to the mid-fifteenth centuries. There is little that is remarkably new in this picture of south German relations with the Hanse in general and with portions of Flanders in particular, with Lombardy (particularly with Milan and Venice), and with Austria-Hungary and the trans-Carpathian region. The great value of this part of von Stromer's work lies in his extensive research in public and private archives, his confirmation of many commercial documents used by earlier scholars, and his discovery of numerous additional documents not utilized by economic historians before this time. The result is to fill out much that has been mainly conjectural heretofore.

The second portion of von Stromer's work is more original and will probably need to be modified somewhat when future scholars have investigated the sources from a different point of view. Here the author is concerned with the

relationship between Nuremberg financiers and the policies (especially the Italian policies) of the imperial Luxembourg dynasty. Undoubtedly the availability of extensive credit as well as financial expertise allowed the members of the Luxembourg dynasty to act with greater confidence and more ambition than would otherwise have been true—and perhaps the ease and availability of such financing contributed to their failure to take a sufficiently realistic view of the peculiar problems of the medieval empire.

The last section of this work is filled with transcriptions of original documents, tables, bibliography, and indexes. Von Stromer's bibliography, even though presented in curiously abbreviated form, is excellent. The various indexes, covering the individual members of mercantile families as well as the great houses and places mentioned in the documents, are most useful. Only the inflated price at which this work sells will detract from its general scholarly appeal.

It is more difficult to fit Professor Schwarzkopf's monograph into the general theme of economic history. Her work is more narrowly the study of the accounts kept by one man, Humbert de Plaine, during a brief period (1448-52) while he exercised financial control at the Burgundian court of the duchess, Isabella of Portugal, and was also master of accounts for her son, Charles the Bold, Count of Charolais. As such, this is a careful study of accounting methods developed in the Burgundian territories and practiced by Humbert de Plaine; but at the same time it offers something a little broader—a study of the social and economic place held by members of those families who had devoted themselves for several generations to mercantile and financial activities. Quite obviously the original humble beginnings of the Burgundian mercantile families did not prevent their entering the ducal service and being rewarded for their services by grants that might bring them into the lower ranks of the feudal aristocracy.

For the student interested in the development of accounting procedures as well as for the social historian interested in the activities and products for which greater or lesser sums of money might be spent in the Burgundian

territories while that conglomerate duchy was at the peak of its prosperity and power, this will be an extremely useful work.

K. F. DREW  
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JOSEPH RATZINGER. *The Theology of History in St. Bonaventure*. [Chicago:] Franciscan Herald Press. 1971. Pp. xv, 268. \$12.50.

This book, a translation by Zachary Hayes of *Die Geschichtstheologie des Heiligen Bonaventura* (1959), is unchanged from the original, as Ratzinger comments that the reviews of his work did not substantially alter his basic thesis, except for some modification of his views of the dependence of Bonaventure on Joachim of Fiore. He analyzes the historical views of "the prince of mystics" through his university sermons delivered at Paris in 1273. These *Collationes in Hexaemeron* were mined in an attempt to discover how the theology of history had changed from the great static concepts of the Church fathers like Augustine to the new dynamic views of the Joachimites.

Accepting much from Joachim's *Concordia Veteris et Novi Testamenti*, Bonaventure was led to agree that "new knowledge arises constantly from Scripture; and this happening, this history, continues onward as long as there is any history at all." Scriptural exegesis clarifies the past and in turn permits prophecy. As Christ is the center of the ages and as all is determined by "that same divine order which is the unifying law of all reality," a theology of history becomes imperative, seeking not only the "meaningfulness of events" but the "eventfulness of meaning." Chaos, randomness, and caprice are thus ruled out of the main stream of human history.

Bonaventure found the source of his inspiration through the progressive reinterpretation of Scripture and from a mystical attempt to ascertain the inner meaning of the Old and New Testaments and their relationship to each other. The Middle Ages were, like most ages, full of change and controversy. The battle raged between the views and conclusions of an Aquinas and those of the advanced mystics. Somewhere in between was the "orthodox" mysticism of Bonaventure. If allegory and numerology, doctrines of six or seven ages of man,

the great chain of being, and the identification of Francis of Assisi with Elijah and John the Baptist cannot be taken for granted by many of today's pragmatic historians, Dr. Ratzinger has nevertheless done all a great service by producing a clearly worked out map of part of the mind of a great medieval thinker.

ALLEN D. BRECK  
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ANTONY BLACK. *Monarchy and Community: Political Ideas in the Later Conciliar Controversy, 1430-1450*. (Cambridge Studies in Medieval Life and Thought, Third Series, Number 2.) New York: Cambridge University Press. 1970. Pp. xii, 189. \$10.50.

From the thirteenth century on, the relationship between the head and members of bodies politic was a recurrent issue in the political life of European societies, and each such struggle for authority occasioned new efforts to define in theory the relation of the ruler to his community. This book, true to its title, examines the theories advanced for and against the claims of the Council of Basel (1431-49) to be the supreme authority in the Church. The work is admirably organized as three concise essays.

Part 1 delineates the views of the Baslean Conciliarists, among whom John of Segovia emerges as both the typical and leading thinker. At Constance the Conciliarists had claimed that the council was in some special cases superior to the pope; most notably the Basleans generalized this claim into superiority in every respect. Among Segovia's political ideas the most remarkable is "the concept of trust," which measures the injustice of government by, as it were, the credibility gap. Thus any *presidens* who acts contrary to the intention of his people, or the majority of that *multitudo*, has in fact lost the trust (*credulitas*) that for Segovia was the basis of authority. Such an evil president is deposed *ipso facto* because, once he has discredited himself by opposing his own private judgment to the will of the majority as duly expressed in a representative assembly, he can no longer be presumed to be a public person who represents the common will.

Part 2 treats the apologists employed by Pope Eugenius IV (1431-47), whose doctrines

are accordingly dubbed "Eugenian Papalism." Led by Torquemada's uncle John (alias Turrecremata), they founded the necessity for all monarchies on a Neoplatonic natural theology in which all earthly authority emanates from God, the supreme hierarch. One wonders whether in this view temporal power descended to kings from the papacy, but presumably the papalists, bidding for royal support, tactfully left that implication carefully folded up.

These two essays may themselves be characterized as an emanation from Walter Ullmann, whose distinctive style of scholarship they eminently reflect. Judged by Ullmannesque standards Antony Black is largely successful, for he achieves a highly selective synthesis, broadly conceived and executed with praiseworthy clarity and concision. But since nonspecialists especially will be attracted by this masterful manner, it is only fair to advise the reader that Black's grasp on scholastic thought before 1350 is not as broad and firm as his frequent bold generalizations might lead one to suppose. His errors, however, are for the most part those *peccata proficientium* that experienced scholars should tolerate in a first work displaying great talent and much effort.

For example, Black overestimates the originality of his Basleans because he does not recognize their debt to Marsiglio of Padua, who had concluded that only a general council has the authority to designate and depose the pope (*Defensor pacis* 3. 2. 32). Concerning deposition we are told instead that "what Marsiglio [*sic*] had already said of the small state, Conciliarism applied to a larger community" (p. 44). Still worse, a Baslean doctrine in which Turrecremata himself discerned Marsilian influence "more probably had a common source in civic and other associations" (p. 54).

The third part, in contrast to the first two, marks a welcome descent from those lofty realms of abstracted theory where we too often make it appear that theories were framed solely for their own sake rather than in response to particular historical circumstances. With a political scientist's keen sense of the interplay of theory and practice, Black shows how, as propagandists and diplomats, the theorists on both sides developed, deployed, and artfully adapted

their distinctive themes in the course of two decades of political maneuver.

One can only hope that Black will not rest content to be a brilliant essayist but will now do for Segovia what Francis Oakley, whose interests resemble his own, has done so solidly for Pierre d'Ailly.

RICHARD KAY  
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JANE E. SAYERS. *Papal Judges Delegate in the Province of Canterbury, 1198-1254: A Study in Ecclesiastical Jurisdiction and Administration*. (Oxford Historical Monographs.) New York: Oxford University Press. 1971. Pp. xxv, 398. \$17.75.

The use of local ecclesiastics as judges delegate was an innovation in the twelfth century that made possible an effective papal jurisdiction over provinces as distant as Canterbury. The legal theory and something of the practice in certain *causes célèbres* have been fairly well known, but evidence for the complete functioning of the system of judges delegate has remained buried in scattered ecclesiastical archives. The author has completed the tedious but important task of ferreting out the cases and abstracting the administrative procedure and has done it well.

The main strength of the book lies in the description of procedure and of the types of cases. In fact, the principal use for the book other than by those persons specifically interested in the subject is likely to be as a handbook to guide other medievalists who are faced with the problem of coping with documents from cases before judges delegate that they may encounter in the course of related researches. In an appendix there are specimens of the various types of documents produced at stages in the procedure that can well serve as comparisons.

As a sometime practitioner of administrative history, I may be permitted the comment that one of the weaknesses of the genre is that the writing often becomes a summary of case after case and instance after instance. Miss Sayers's work is no exception, with entire chapters pounding relentlessly on as a slightly expanded and heavily documented outline. The reward from this meticulous scholarship is only partially forthcoming, for the author has a marked reluctance to develop general conclusions. Her



two pages of formal conclusions, even when supplemented with some general remarks in the introductory pages, can do little more than suggest some of the broader questions on which her views would have been valuable.

It is true that F. W. Maitland and Z. N. Brooke showed that England was an integral part of the ecclesiastical jurisdiction of Rome, but how did the use of judges delegate contribute to the growing power of the papacy? Although mentioned, the parallel appointment of papal legates is also not fully explored in relation to judges delegate. Most of all, the book lacks a sense of development or emphasis on trends that would allow a reader to understand why Archbishop Pecham in 1284 called appeals to Rome one of the major abuses of the Church.

The author's opinion that ecclesiastical courts drew cases away from the secular courts is startling. If this is so, the situation had changed since the days when Ranulf Glanville twitted Walter Map about the inefficiency of ecclesiastical justice. The major disappointment is that so little emphasis is placed on development that the book would not provide an answer either way on this question of efficiency, even though the question is central to an evaluation of the overall effect of the practice of using judges delegate.

CHARLES R. YOUNG  
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J. A. WATT. *The Church and the Two Nations in Medieval Ireland*. (Cambridge Studies in Medieval Life and Thought, Third Series, Volume 3.) New York: Cambridge University Press. 1970. Pp. xvi, 251. \$13.50.

This solid volume deserves to take its distinguished place with other scholarly monographs in the Cambridge Studies in Medieval Life and Thought. The author handles his difficult subject, the Church in Ireland through the twelfth, thirteenth, and fourteenth centuries, with detachment and reserve—virtues that other scholars who have written about that troubled island have not always possessed. What renders the practice of these virtues imperative here is, first, the sensitivity of the subject, and, second, the care with which the limited and frequently uncertain documentary evidence must be examined. So we find the au-

thor commiserating with himself over his task: "There is no more difficult problem in medieval ecclesiastical history than to know the mind of an Irish bishop" (p. 147).

The reader is inclined to agree with Dr. Watt that Henry II's invasion of Ireland was "the most important event in the history of the Irish Church between the fifth and sixteenth centuries" (p. 40). The author also makes a strong case for his view that the Church in Ireland, which he believes may have been on the threshold of beginning its own reform, was destined because of that invasion to remain throughout the Middle Ages "a remote and backward province of the Church, stunted in its growth, distorted in its development" (p. 2).

What blighted the expectations of the papacy—which approved English *dominium* of the island—and of the Irish hierarchy, which appears initially to have acquiesced in that rule, was a dormant nativism that the intrusion of the foreigner caused to surface. So acute did this antagonism between Irish and English grow that religious orders as dedicated to the spiritual ideal as the Cistercians and Franciscans found it impossible to accommodate both Irish and English in the same communities. The Crown, whose principal concern was political control, did not help matters by applying the principles of common law as they touched the clergy and by demanding the same voice in the selection of bishops in Ireland as it enjoyed at home. What might have proved a stronger unifying force than a common religion was royal taxation, and had Edward I and his successors pressed their fiscal demands with greater harshness and persistence, time might have served to dull the sharp animosities that divided the Irish, the Anglo-Irish, and the English clergy.

Although Dr. Watt leaves the reader with a bleak picture of the Church in Ireland, he introduces on the way a number of fascinating Irish churchmen whom, one hopes, he and other scholars will shortly bring into fuller view.

JOSEPH DAHMUS  
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RÉGINE PÉRONOUD. *Héloïse et Abélard*. Paris: Éditions Albin Michel. 1970. Pp. 298. 19.50 fr.

This is an extremely fine book that is well



worth reading on several counts. Using the story of Héloïse and Abelard as her focus, Miss Pernoud has recreated much of the complexity and diversity of the thought world of the twelfth century. In her technique she belongs to the family of historians that includes Christopher Brooke; she is steeped in the milieu she describes, and because it clearly has an immediacy for her she is able to convey her sense of the period vividly to the reader.

Abelard's education, the full range of his scholarly development, and his philosophical ideas are presented very lucidly; but rather than seem like old ground gone over again, Pernoud catches the intellectual excitement of the period. Abelard's passion for dialectic becomes comprehensible in human terms, though humanizing Abelard does not in any sense detract from Pernoud's portrait of him as the greatest genius of his era. She seems, in fact, to agree with Abelard's assessment of himself—an assessment that should probably be slightly modified. But the key to his personality and character, as Pernoud presents it, lies in the fact that his extraordinary intellectual drive was the main force in his life. Seen in this way, his love for Héloïse was basically his response to her unusual intellectual gifts.

The portrait of Héloïse is beautifully drawn and the love story is told very much from her point of view. An interesting and tragic tension existed between Héloïse and Abelard because she was passionately in love with him but able to perceive his shortcomings as an emotional being. Pernoud compares her to Simone de Beauvoir and sees a parallel between de Beauvoir's reasons for not wanting to marry Sartre—her reluctance to see him become "professeur en province et, définitivement, un adulte"—and Héloïse's wish not to marry Abelard. But Héloïse still belongs to the twelfth century, and her devotion to Abelard and the way she saw their love was colored in good measure by the ideals of courtly love. She was a woman of real spirit—both literally and figuratively.

I consider it appropriate (and probably not an accident) that a woman was asked to review this book, because I can say with confidence that in it Héloïse really has her day.

JILL N. CLASTER  
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PETER LLEWELLYN. *Rome in the Dark Ages*. New York: Praeger Publishers. 1971. Pp. 324. \$10.00.

In this brief account Rome refers primarily to the capital city, and the dark ages encompass the centuries between 476 and 962, from the days of King Theodoric the Ostrogoth to the coronation of Emperor Otto I. Though Italy and the Empire come under discussion, the argument basically focuses upon the continuity and the eventual victory of papal Rome. The book is well written, and the author intersperses his narrative with colorful translations from the sources; however, the volume is compact, replete with unfamiliar names, and not easily read.

After a sympathetic account of Theodoric, the author relates the reconquest of Italy by the Byzantine forces of Belisarius and Narses. It is an exciting account that arouses sympathy and admiration for the defeated Ostrogoths and disdain for the Byzantines and Romans. The reconquest brought no great relief to Rome and Italy; instead Franks, Lombards, plague and floods, and the inept administration of many Byzantine officials decimated the population and impoverished the survivors. In perilous times Pope Gregory I assumed the political and spiritual leadership of Rome and much of the peninsula. Not until the eighth century did the Roman popes rise high above the machinations of Byzantines, local dukes, and archbishops. This chapter, "Rome and the Byzantine Empire," is very convincing. In "Rome and the Pilgrims" the author very ingeniously uses the pilgrims to indicate the architectural changes from imperial to papal Rome and likewise to extend Roman ideas and practices northward and westward.

The victory over the Byzantines left only the Lombards and the Roman noble factions. The struggle involved intrigue, treachery, and murder; in the end the Frankish support against the Lombards saved Rome and papal authority. These successes did not always guarantee papal control in Rome itself where political factions resorted to the most nefarious activities to dominate the papal office, and they often succeeded. Even the close alliance between Leo III and Charlemagne and the imperial coronation of that monarch did not eliminate completely the factional threat. In fact the

deterioration of Carolingian authority permitted the return of factional control that brought disaster, ignominy, and the intervention of Otto I in 961.

As a political, diplomatic, and military narrative the volume is convincing. I hope that the author will offer a similar account in the pastoral, social, and economic sphere to complete his arguments.

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University of Cincinnati

GABRIELLA ROSSETTI. *Società e istituzioni nel contado lombardo durante il Medioevo: Cologno Monzese*. Volume 1, *Secoli VIII–X*. (Archivio della Fondazione Italiana per la Storia Amministrativa. First Series, Monografie, ricerche ausiliarie, opere strumentali, Number 9.) Milan: Dott. Antonino Giuffrè. 1968. Pp. 242.

Miss Rossetti has presented us with a mass of detail concerning the history of Cologno Monzese and its environs, a rural area of the Milanese contado, during the eighth through the tenth centuries. It requires some patience to wade through a cluttered presentation, but Miss Rossetti has drawn from the welter of place and proper names (to which an index is mercifully provided) certain conclusions of interest to the economic and social historian.

Leaving aside the subordinate arguments and myriad problems of interpretation, the one recurring theme of the work is the economic penetration of Cologno by the Milanese monastery of Saint Ambrose. Although Cologno remained socially Lombard throughout the period under consideration, by the ninth century Lombard landowners had fallen upon hard times. The division of the patrimony among male heirs had resulted in breaking up the relatively large unitary familial holdings of the preceding century into small and economically marginal farms. The coming of the Franks and feudalism further aggravated the economic situation of the Lombard population of Cologno since the Franks occupied positions of power and burdened the land with a variety of dues. Into this situation stepped the abbots who pursued, from the mid-ninth century, a seemingly continuous policy in Cologno of putting together unified blocks of land. They secured land both by donation and foreclosure, but they apparently relied most heavily upon ex-

change as the means to their end. In exchange for ground contiguous to their existing holdings in Cologno and elsewhere in the contado, the abbots traded ground in Milan and sites in the castle of Cologno, which had been built under their aegis and jurisdiction in the mid-tenth century. This policy drew a scattered Lombard population to the castle, which subsequently acted as a social and economic focal point for the region. At the same time it stimulated immigration from Cologno to Milan as Colognese exchanged patrimonial parcels for sites in the city. Thus, those able to immigrate were from the relatively well-to-do class of landowners who, when in the city, turned to ecclesiastical or legal careers as entrees into urban life.

Such a picture of monastic penetration of Cologno and its consequences overlooks a number of important questions. For example, how were these acquired lands exploited? Were they leased or worked directly within the larger framework of monastic holdings? Furthermore, the subject is slighted of emigration from the city in the form of investment as urban merchants, ecclesiastics, and others purchased rural land. Miss Rossetti relegates this important aspect of the history of Cologno to a footnote, although she promises to take it up in the sequel. Still, we know from Professor Cinzio Violante's study, *La società milanese nell'età pre-comunale* (1953), that of the names of the holders of adjacent properties mentioned in the Colognese documents of the beginning of the eleventh century, twenty-eight were lay and but twenty-one ecclesiastic. In any event, should not this counterflow from city to country have been examined within the context of the present volume and thus counterbalance the impression of an all-conquering monastery gobbling up the lands of a depressed peasantry in Cologno Monzese?

Miss Rossetti promises a second volume. We may look forward to this with the hope that she will undertake a greater effort at synthesis and that she will cast her results in a less bewildering form.

THOMAS W. BLOMQUIST  
Northern Illinois University

ALFRED HAVERKAMP. *Herrschaftsformen der Frühstaufer in Reichsitalien*. Volumes 1 and 2.

(Monographien zur Geschichte des Mittelalters, Number 1.) Stuttgart: Anton Hiersemann. 1970; 1971. Pp. 311; 314-813, 4 maps. DM 79; DM 98.

This is a thorough and systematic survey of the administration of Italy by the early Staufen emperors. Although it concentrates on institutions rather than on people, it does, at long last, for Italy what Bosl's *Reichsministerialität* (1950) has done for Germany. In two volumes, after a discussion of the views of earlier historians and a statement of the methodological principles involved, the author gives an exhaustive treatment of the whole question of *regalia*, of the institutions of vassalage, and, finally, of the financial policies. For the charters he is still using Stumpf-Brentano's *Die Reichskanzler* (1865-83), and it remains to be seen whether the long awaited editions of the charters for the *Monumenta*, the first volume of which is now in the press, will provide material that will force a vital revision of our knowledge in general and of any of the conclusions reached in the present book, in particular. The author's learning and thoroughness are impeccable and will make the work indispensable. Nevertheless, the critical reader must express some weighty doubts about the general impression created by the work. The author's opening sentence is significant: "The government of the Staufen which lasted for more than one hundred and fifty years. . . ." With this opening he conveys the impression that the rule of the Staufen was a systematically organized government, the features and outlines of which are clearly discernible. In reality, except for the brief years of Frederick II in southern Italy, the Staufen rulers struggled for power and dominion, using different methods at different times in different parts of their geographically diverse and far-flung dominions. It is, after all, no accident that the ascendancy of the Staufen dynasty ended in the *interregnum* and that their multifarious administrative experiments finally sealed the fate of monarchy in Germany, at least in the sense in which monarchy went from strength to strength in France and England. Although the author is sensitive to the many changes, he presents a static picture of systematic administration rather than a long series of political compromises, expedients, and improvisations. Dominated by the general conception expressed in the opening lines, the

author seems to misinterpret the fundamental variety of the changes and administrative measures. Thus he says, for instance, on page 79 that the achievements of the Peace of Venice were due to "masterly diplomacy on the part of the imperial government." It is difficult to associate any clear fact with the expression "imperial government." There was no question of government at all. At that time Frederick and his paladins were sitting pretty in northern and central Italy and tried to make the most of the fact that they wanted peace with the Lombards while Frederick's military situation in Lombardy itself was mediocre and in central Italy above average. In reality, at the moment, Frederick was conceiving a new plan of exercising power, and there seems much less institutional continuity between the policies before the peace and the policies after the peace than the author lets on. Similarly, he glosses over fundamental changes and thus exaggerates institutional continuity at the expense of reality by saying on page 734 that in the sixties the imperial government becomes more effective through the use of imperial legates and that their use amounted to an "institutionalization" of imperial dominion in Italy. This is not only an overemphasis on institutional continuity but also a distortion. For we know that Frederick's initial plan for Lombardy came close to what modern historians of Africa have called "indirect rule" and that the employment of imperial vicars was resorted to as an expedient forced upon Frederick after the destruction of Milan in 1162. The ascendancy of the vicars can hardly be questioned. But their rapaciousness and tyranny was the immediate cause of the ultimate rebellion of the cities, and one can no more describe their administration as an increased effectiveness of imperial government than one can consider their existence as a natural growth of Staufen "government." These shortcomings of the author's general picture are conditioned by his method. Instead of writing history by plotting the course of events through the years, he has attempted an institutional survey. Of necessity this method emphasizes institutional continuity and system at the expense of compromises and measures forced upon the various participants by the day to day struggle for power. Although these criticisms

are major ones, this book will remain the standard work on the subject.

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AUGUSTO VASINA. *Romagna medievale*. Ravenna: Edizioni A. Longo. [1970.] Pp. 360. L. 4,000.

This volume is a useful collection of Augusto Vasina's varied studies published over the last decade on life in the Romagna. In them he considers problems of the Church, politics, and culture during the Middle Ages and the Renaissance. His familiarity with archival sources allows him to treat the ecclesiastical history of Ravenna and her neighbors in detail; observations on the triumph of localism and the replacement of Byzantine and Roman clergy by natives is indicative of a trend. He discusses problems of episcopal administration, stressing the role of the bishops as they sought to re-establish the order and authority of the twelfth-century Church. Control over canons of the cathedral was essential both for episcopal administration and liturgical practices. In those troubled years severe contests erupted between various local churches and episcopal authority. Frequently the nascent urban commune identified with the local churches enabling the population to unite against foreign bishops.

Vasina adopts Cinzio Violante's explanation that the commune was not born from a fierce confrontation between bishop and citizenry; instead, it emerged as a consequence of a power vacuum, with public works and defense being handled by the urban patriciate. My impression is that both Vasina and Violante have moved too far against the traditional class interpretation. So keen an observer as Pier Damiani made distinctions between the greater and lesser citizens noting that their conflict was a force for change. Surely the power of the bishops was weakened as concessions were made to the nobles; by the same token, however, the newer elements in society were anxious to win a greater share of the spiritual benefits of the late medieval world.

Political struggles in communal Italy have a spiritual dimension. By the early fourteenth century localism had triumphed and the papacy was unable to control the Romagna. In

this political milieu Vasina observes that the municipal historical tradition of the late Middle Ages oscillated between a moralistic condemnation of factionalism and an apologetic tendency toward the great clans. Of these the author writes zestfully of the Malatesta of Rimini. So little attention has been given to the formation of Renaissance courts and the transformation of itinerant *condottiere* into arbiters of elegance that our understanding of the period runs a risk of being too dependent on the history of a few flourishing republics. Vasina shows that the horizons of provincial families opened during the fifteenth century and that the Renaissance court was no mere continuation of its medieval counterpart. In fact, the differences are so striking that we must develop a fresh historical language to describe the emerging culture. The author makes the telling point that in Rimini the precariousness of the life of the courtiers and their lord was camouflaged by opulent drapes, extravagant clothes, silks, and decor. Perhaps the Renaissance passion for enumerating items of luxury, particularly jewels, was connected with this sensibility. About all this we need to know much more; after all, court painters celebrated simple domestic bliss as well as exotica and erotica.

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WOLFGANG WEHLEN. *Geschichtsschreibung und Staatsauffassung im Zeitalter Ludwigs des Frommen*. (Historische Studien, Number 418.) Lübeck: Matthiesen Verlag. 1970. Pp. 143. DM 20.

Wolfgang Wehlen defines this study as an investigation into the concept of state prevailing in the era of Louis the Pious as reflected in two historical works, Nithard's *Historiarum libri quattuor* and Paschasius Radbertus's *Epitaphium Arsenii*. In fact his concern is considerably narrower; he concentrates on explaining what these two historians meant when they used the term *res publica*.

In preparation for his treatment of Nithard and Paschasius Radbertus, Wehlen devotes the first third of his book to a survey of the meaning given to *res publica* by various authors from the sixth to the ninth century. Although this survey reflects a thorough investigation of

many sources and a careful cataloging of diverse applications of the term, it is not entirely satisfactory. Wehlen fails to convey any general sense of what *res publica* meant to ninth-century men concerned with the nature of the state largely because he refuses to inject himself into the discussion as interpreter, summarizer, and synthesizer.

I therefore proceeded to the treatment of Nithard and Paschasius Radbertus without a firm sense of the ideological context in which they operated. Wehlen's analysis of his two documents is more rewarding. He carefully sorts out the uses made by each author of the concept *res publica* and explores in precise terms the sense they gave to the term. His investigations will provide students of Carolingian political theory with considerable help in identifying and defining certain elements of the vocabulary of political thought and with valuable insights into the major concerns of those interested in the ninth-century state. In a larger sense, Wehlen establishes that both historians found in the term *res publica* a "programmatischer Begriff," an ideal against which they could evaluate the political difficulties of the era stretching from about 830 to 843. Wehlen's study has the added advantage of being based on two documents that deal with actual historical events, thereby permitting the reader to see how at least two Carolingian writers applied theoretical concepts to concrete situations.

The work, however, has serious limitations. Wehlen's technique of piling quotation upon quotation, apparently based on the assumption that the sources speak for themselves, is not conducive to a clear understanding of the ideas behind these quotations. The task he sets for himself demands a greater attention to analysis and explication. Nithard and Paschasius Radbertus stand isolated in their age because of Wehlen's failure to relate their ideas effectively to those of their predecessors and contemporaries. Given the fact that both were partisans and apologists (Nithard for the cause of Charles the Bald and Paschasius Radbertus for the "Einheitspartei" and Lothair), one is never certain whether either is seriously concerned with the nature of the state in a theoretical sense. Wehlen's fixation on their use of the concept *res publica* delimits any concern with a larger view of the political consciousness of his authors. Fi-

nally, the study does not effectively establish the critical point that *res publica* was a fundamental concept in the political ideology of the early ninth century. At least I felt that the perspective of the study was too narrow to permit a fuller understanding of what the title promised: "Staatsauffassung im Zeitalter Ludwigs Frommen."

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KURT-ULRICH JÄSCHKE. *Die älteste Halberstädter Bischofschronik*. (Mitteldeutsche Forschungen, Volume 62, Part 1. Untersuchungen zu mitteldeutschen Geschichtsquellen des hohen Mittelalters, Volume 1.) Cologne: Böhlau Verlag. 1970. Pp. vii, 238. DM 54.

Some twenty years ago Helmut Beumann, an intellectual historian at the University of Marburg with a special interest in medieval historiography, wrote a brilliant interpretative study of Widukind of Corvey, which, alas, went unnoticed in these pages. He is now editing some studies on the historians of central Germany during the High Middle Ages, of which the volume under review is the first. This book by Kurt-Ulrich Jäschke is a painstaking attempt to determine content and structure of earlier, nonextant versions of the chronicle of Halberstadt, a work of the thirteenth century. Beyond that it seeks to establish whether the growth of Church organization in central Germany during the later tenth century found an echo in the historiography of the period. The author rejects the assumption shared by an older school of historians that Thietmar of Merseburg (d. 1018) had been the first to write history in central Germany and that his work was the source for all subsequent historiography in that region. He also refutes a more recent thesis of Bernhard Schmeidler, who claimed that nobody wrote history in such ecclesiastical centers of the German frontier as Magdeburg, Nienburg, and Halberstadt before the twelfth century. In an amazing application of the great-man theory, Schmeidler made Abbot Arnold of Nienburg and Berge, who lived during the middle of the twelfth century, the first historian in this area and the author of an array of local annals and chronicles. Instead Jäschke returns to the view that the origins of these works are diverse and complex.



More specifically he advances the thesis that an influential prototype of the episcopal chronicles of Halberstadt was composed as early as 992 or 996.

The historiographical revisions proposed in this meticulous study may seem less earthshaking to a reader on this side of the Atlantic than to a German medievalist not so many miles from the scene; one might even feel, and wrongly I believe, that here a mountain of rigorous medievalism has given birth to a revisionist mouse. True, there is an air of unreality about discussing authorship, motive, impact, or popularity of works whose being and nature are postulated on the fragile foundation of deduction and hypothesis. Yet from a methodological perspective Jäschke's closely reasoned and heavily annotated book cannot be faulted: the microscopic examination of the texts, the cautious establishing of complex textual interdependencies, and the exhaustive recourse to every conceivable clue, especially charter evidence and cursus, make this an important work, though difficult to read.

BERNHARD W. SCHOLZ  
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GEORGES and DÈMÈTRIOS TORKIKÈS. *Lettres et discours*. Introduction, text, analyses, translation and notes by JEAN DARROUZÈS. (Le monde byzantin.) Paris: Éditions du Centre National de la Recherche Scientifique. 1970. Pp. 381. 86 fr.

This is a useful book, useful not only for the texts it offers, but also for the prosopographical information it contains. The brothers Tornikes were Byzantine functionaries, and to a degree also intellectuals, who lived in the twelfth century. George, the eldest, made his career in the Church, becoming eventually bishop of Ephesus. This is the first complete edition of his letters and discourses. Demetrios, George's younger brother, was a functionary of the state who achieved the position of logothete during the reign of Isaac II Angelus. There are three letters by him included in the present book.

The main body of the book consists of the Greek texts and a summary in French of their contents. Four texts, however, George's long encomium of Anna Comnena, his letter to the Pope, and two other letters to the Pope by Demetrios, are accompanied by translations in full. There are some notes, but more important

is the long introduction where such problems as the careers of the authors, the chronology of the composition of the texts, and their manuscript tradition are treated and where a detailed analysis of the family background of the men to whom the letters were addressed is given.

Byzantine epistolography is highly rhetorical, difficult to read, and very often barren of any concrete information. There may be a few exceptions to this generalization, but the correspondence of the Tornike can hardly be one of them. Nevertheless, we do learn something: the decadent state of the city of Ephesus; the ruinous conditions of its famous church of St. John the Theologian; the reluctance of ecclesiastics, domiciled in Constantinople to accept provincial bishoprics; the differences that separated the Greek and Roman Churches. We learn something also about Anna Comnena, perhaps the greatest woman intellectual of the Middle Ages, both East and West. The encomium of Anna by George Tornikes is no doubt the most important text offered by the present book. Composed several years after her death, it describes at length her intelligence, education, intellectual activities, and other experiences. An encomium can hardly be said to be objective; at best it tells only one side of the story. In this encomium, for instance, the author passes over in silence the political ambition of Anna, an ambition so intense that it involved her in plots against her emperor brother. Nevertheless for the study of the life of that remarkable Byzantine woman and of the intellectual currents in Byzantium during the first half of the twelfth century, this encomium, despite its rhetorical declamations, is invaluable.

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#### MODERN EUROPE

PETER G. BIETENHOLZ. *Basle and France in the Sixteenth Century: The Basle Humanists and Printers in Their Contacts with Francophone Culture*. [Toronto:] University of Toronto Press. 1971. Pp. 367. \$12.50.

This survey complements Bietenholz's previous work on Italian humanism and the Basel print-



ing industry. Its goals are limited: Bietenholz begins by assuring that no extant studies "will be duplicated, and none will be made obsolete" by his book (p. 19), and concludes that he will "stop well short of a complete survey" of Franco-Balois relations (p. 242). Furthermore this volume will be followed by another that has been set aside for an analysis of books and for a number of lengthy *pièces justificatives*. His volumes attempt to see how far the culture of Erasmian Basel and its influence formed a "recognizable factor in the composite tissue of the sixteenth-century French mind" (p. 20). His primary mode of approach is a meticulous examination of Francophone participation in Basel's printing industry from the 1470s until 1650.

The result is an interesting but uneven compilation in which the whole never quite equals the sum of its parts; the synthesis has been deferred for his second volume. Bietenholz has quantified his subject excellently in his first part, "France and Printing at Basle," especially on pages 51-53 (one wishes that the divisions by subject matter were not buried in a footnote). The connections between France and Basel reached a visible peak between 1520 and 1555, the age of Erasmus and of his Balois epigoni, most attractively represented by the Francophone exile Castellio. This section is supplemented by a valuable short-title bibliography (pp. 249-336) that lists the thousand-plus titles on which his study rests. While some of these entries make one wonder about Bietenholz's definitions (for example, why were Jakob Sturm, George Buchanan, Peter Lombard, or Johannes of Segovia "Francophones"?), the principal French authors published at Basel emerge clearly: Castellio leads with 55 entries, followed by Ramus (35), Gilbert Cousin (33), and François Hotman (31). And sometimes the absences are equally revealing: Bodin has only three entries and Calvin five (including of course the original *Institutes*).

Bietenholz's two other sections are uneven. "Francophone Expatriates at Basle" succeeds very well when discussing the French church, Castellio, Baudouin, or Ramus, while "The Focal Contacts in France" succeeds best with Francis I's court or the post-1550 *politiques*. Many other sections, however, are a largely unconnected, discontinuous series of notes, from

which only a few memorable vignettes, like the portrait of the Bauhins (pp. 63f), emerge.

E. WILLIAM MONTER  
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DAVID C. STEINMETZ. *Reformers in the Wings*. Philadelphia: Fortress Press. 1971. Pp. viii, 240. \$8.50.

Most studies of the Reformation, whether written by sympathetic or hostile historians, focus on the major Reformers such as Erasmus, Luther, Müntzer, Zwingli, or Calvin. It requires originality to devote an entire book to the second line of reformers, "the reformers in the wings, the supporting members of the cast who by their lesser and often unnoticed activity furthered the course of the drama to its final curtain." Such a study is facilitated by the fact that some of these reformers were powerful thinkers in their own right. Reading this book, one wonders why it was that the early sixteenth century produced such an amazing number of original theologians.

Devoting an essay of eight to twelve pages to each of the reformers, Mr. Steinmetz presents his study in an attractive and lucid form. Altogether twenty reformers are presented, arranged in four major groupings: late medieval Catholic Reform (Geiler, Staupitz, Contarini, Stapulensis, and Pole), the Lutheran tradition (Melancthon, Bugenhagen, Osiander, Amsdorf, and Brenz), the Reformed tradition (Bucer, Bullinger, Hooper, Vermigli, and Beza), and the radical reformers (Carlstadt, Schwenckfeld, Hubmaier, Denck, and Marpeck). Although the reformers discussed are men of very diverse tendencies, the author skillfully avoided the danger of publishing only a collection of disparate essays. Briefly sketching the life and actual work of these men, Mr. Steinmetz, a theologian, concentrates on the theological issues, thus providing a connecting link between the essays. Indeed, the essays offered him the opportunity to introduce practically all significant issues of sixteenth-century theology: the Nominalist background; the problem of the literal and spiritual interpretation of the Scripture; the relationship between Old and New Testaments; the covenant theology; and the various conflicting doctrines on justification, predestination and free will,

the Lord's Supper, the teaching office of the church, and the relationship between Church and state. Although he bases most of his essays on secondary works, Mr. Steinmetz does use the writings of Staupitz, Melancthon, Carlstadt, Schwenckfeld, and Hubmaier. A useful bibliography follows each essay.

As Mr. Steinmetz's book offers an overall study of a variety of colorful personalities and complex theological topics, it may be used to great advantage in courses on Reformation history. Most textbooks mention these reformers only in passing. On the other hand, students generally do not have the time to read the voluminous works that have been written on these reformers. Mr. Steinmetz's readable and attractive book will fill a definite need. This is not to say that it is without minor flaws. I do not understand why the essays on Geiler and Bugenhagen were included, for they do not tell much about either man. Should these essays have been added to the four sections only for the sake of symmetry?

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W. FRED GRAHAM. *The Constructive Revolutionary: John Calvin & His Socio-Economic Impact*. Richmond, Va.: John Knox Press. 1971. Pp. 251. \$7.95.

Professor Graham presents an interpretation of John Calvin as a revolutionary leader and thinker, perfectly aware of the social and economic implications of his own teachings and deeply involved in the problems of the beleaguered Genevan republic. To the accepted portrait of Calvin as chief architect of a theology that was revolutionary in its own time, Dr. Graham adds and explores the perspective of Calvin as an activist, and a secular, revolutionary leader. While the author sees the origin of Calvin's social views in the reformer's own theology, it is the non-nihilist revolutionary who "wanted to uproot evil and usher in a new age based on the old, without destroying society" that is emphasized. Searching the records of Geneva's municipal councils, the Venerable Company of Pastors, Calvin's works, and a host of relevant secondary sources, Professor Graham has effectively documented most of his as-

sertions. Calvin's struggles with money lenders, merchants, grain speculators, city bureaucrats, the cost of living, wage and price controls, education, food and medical care for the poor, and the staggering influx of refugees are presented with commendable clarity and brevity; the portrait of the involved activist is indelibly drawn. But Professor Graham is consistently judicious, and Calvin's shortcomings are weighed with the rest. He does not hesitate to describe Calvin's nearly merciless repression of anyone he considered a threat to the internal discipline of the community; in fact the author suggests the theological origins of this behavior in Calvin's Christology. It is a remarkably comprehensive and convincing case for so brief a volume. The perennial Weber thesis controversy is treated with unusual accuracy, sophistication, and originality; but some of the author's other suggestions are perhaps less likely to carry his readers along. Probably the most fertile sources of comment will be such assertions as "history discloses how men make their own future," and Calvin's career is an example of "how revolutionary leaders have helped man in the past." But Dr. Graham is a capable advocate, and a succession of potentially obscure topics are skillfully dealt with in eminently readable prose. The handsome dust jacket, however, is an inadequate substitute for a clear black and white map or schematic view of Calvin's Geneva. For the general reader as well as the scholar this volume merits wide circulation.

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*Studia Leibnitiana: Supplementa*. Volume 4, *Theologie; Ethik; Pädagogik; Ästhetik; Geschichte; Politik; Recht*. (Akten des Internationalen Leibnitz-Kongresses, Hannover, 14.-19. November 1966.) Wiesbaden: Franz Steiner Verlag GmbH. 1969. Pp. vi, 263. DM 44.

In 1966 a Europe forever purged, we may hope, of arrant nationalism by the ordeals of the twentieth century paid generous tribute to one of her most celebrated ecumenical spirits when she marked the two hundred and fiftieth anniversary of the death of Leibniz. Commemorative observances in Paris, Geneva, and Milan culminated, appropriately enough, in an international congress held in Hanover from November 14 to November 19. The Leibniz

Gesellschaft instituted on that occasion commissioned a new quarterly, the *Studia Leibniana*, which first appeared in 1969. A vehicle for the publication of longer monographic studies has also been provided in the form of supplementary volumes to the *Studia*. The first five of these, however, contain the addresses presented at the 1966 congress, with volumes 1-3 devoted to philosophical and scientific subjects and volume 5 to the history of philosophy.

Volume 4 of the *Supplementa*, with its sixteen *Vorträge* associated with seven titular rubrics, is especially wide ranging. The notorious multifariousness of Leibniz's ideas and activities requires no comment, but it is well to remember that a philosophical system based on the notion that individual beings are at once infinitely varied, interrelated, and expressions of an ultimate unity offers certain problems for the scholar who wishes to investigate any particular aspect of the system. And the difficulty is more rather than less severe in the case of the student of Leibniz's historical work, for here the empirical side of his method tends to disorganize, though by no means to discourage, his search for progressively more inclusive "unities-in-variety."

Of those contributors to the volume under discussion whose subjects are historical, some are content with one dimension while others, as a group more ideologically inclined, pursue Leibniz the "panhistor," but all understand Leibniz's profound interest in history, which is sometimes overlooked in general works. Eduard Winter sees Leibniz's *Kulturpolitik* as bound up on the conceptual level with the *Grundgedanken* of harmony, universality, continuity, and progress, as functionally related to the advance of education along many lines, and as geographically manifested in a kind of world civilization. Waldemar Voisé keeps his panhistor busy explicating several relationships: that linking the past, present, and future in the historical process; the bond between history and politics that might serve as the basis of a "science of a new sort"; and, very nearly (as well as somewhat extravagantly), the intimate affiliation of politics and statistics. Günter Scheel follows in detail the elaboration of Leibniz's plan for a typically monumental *Opus historicum*, which was, just as typically, never

brought to realization. The political ideas of Leo Stern's dialectically bedeviled panhistor lead to the depiction of an ideal world that is to issue from the reconciliation of competing interests, though these ideas originate in reflections upon the political problems of the day and especially in Leibniz's famous attempt to achieve a reunification of religious disciplines. This last subject is approached from different points of view by Jean Guitton and Lotte Knabe, but both predictably make the connection with the current ecumenical movement. The panhistor is demoted to *Apollo antisaxonicus* by Jean Neveux and merely watches as Paul Wiedeburg ably investigates a statement alleged to have turned Louis XIV against Leibniz's Egyptian Plan.

Many signs indicate a promising prospect for Leibniz studies: the abiding interest of a genuinely international group of capable scholars—and this country is very honorably represented; the modest but pleasing progress made on the German Academy edition of Leibniz's complete writings; and the plans made by the Leibniz Gesellschaft for another congress in the summer of 1972. And for this the 1966 meeting set a worthy standard.

WALTER J. BRUNHUMER

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RAGNHILD HATTON and M. S. ANDERSON, editors. *Studies in Diplomatic History: Essays in Memory of David Bayne Horn*. [Hamden, Conn.: Archon Books. 1970. Pp. xiv, 384. \$11.00.

The editors of this volume of essays in honor of the late Edinburgh historian D. B. Horn permitted considerations of persons to mar its thematic integrity as set forth in its title. Three of the eighteen historical essays (Denys Hay writes an introductory chapter on Horn himself) have little or nothing to do with diplomatic history, although Dietrich Gerhard's piece on regionalism and corporate order in European history (first published in *Historische Zeitschrift* in 1952) makes one wish that he had turned his particular gifts of erudition, imagination, and incisive thought toward the general problem of the states system. The essay by Guido Quazza is for the most part a sketch of the internal history of the Italian states in the first half of the eighteenth century; C. A. Macartney contributes a close study of the pre-

cise character and origins of the Austro-Hungarian Compromise of 1867.

All the other essays are on themes of diplomatic history, and most lie in the period between the Peace of Westphalia and the French revolutionary wars, the period in which Horn himself did most of his work. Some are concerned with episodes that have been little known or at best misunderstood, and they rest upon fresh archival research as well as wide-ranging command of printed sources. Notable among these are the study by Andrew Lossky on Dutch diplomacy and the trade negotiations between France and Russia in 1681, built around the correct identification of a document twice printed in the nineteenth century but wrongly attributed; Ragnhild Hatton's own contribution—a study of John Drummond, an English merchant turned diplomatic agent in Holland during the War of the Spanish Succession; and Stewart Oakley's account of the thwarted plans of Gustavus III for war with Denmark in 1783–84. The essay by G. C. Gibbs on the laying of treaties before the British Parliament in the eighteenth century links diplomatic history with domestic constitutional developments in a way significant for both, as does the study by Henry L. Snyder of the problem of diplomatic appointments during the Godolphin ministry for domestic political history. V. G. Kiernan lightens the fare with wry anecdotes of the small fry of diplomacy, careerists of greater ambition than influence. M. S. Anderson's discussion of theories of the balance of power in the eighteenth century—one of the key problems of European diplomatic history—is informative and thoughtful.

The volume is rounded off by a characteristically sagacious essay by Sir Herbert Butterfield on the significance of diplomacy in the relations of states and peoples; he observes in particular that diplomatic negotiations are not essentially efforts at persuasion but the verbal equivalent to war, in the Clausewitzian sense of the imposition of one will upon another—but better than war for all that. The other essays by James N. M. Maclean, John C. Rule, Walther Mediger, Alice Carter, Michael Roberts, I. H. Nish, and Esmonde Robertson display the same qualities of excellence of research, intelligence, and perceptiveness. The absence of nationalist passions should not go

unremarked, all the more when it is accompanied, as in these authors, by an awareness that what was at stake in the episodes they study was not the private affairs of the statesmen, great or small, but the fate of peoples.

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New Brunswick

PIERRE CHAUNU. *La civilisation de l'Europe des Lumières*. (Collection Les grandes civilisations, Number 11.) [Paris:] Arthaud. 1971. Pp. 664.

Within the past five years, and in some instances perhaps prematurely, increasing numbers of quantifying historians in France have turned aside temporarily from their magisterial regional studies of *ancien régime* society and have undertaken striking syntheses intended for the nonspecialist and the cultivated public. Often applying to the archival and statistical findings of an entire generation an intuitive genius worthy of the heirs of Marc Bloch and Lucien Febvre, scholars such as Fernand Braudel, Pierre Goubert, Pierre Léon, Robert Mandrou, and Pierre Chaunu have written a remarkable series of essays. They sweep confidently across France, Europe, America, and Asia of the sixteenth through eighteenth centuries, and guarantee for this period the central place of a social and economic interpretation in the widest sense. On the right wing of present-day *annaliste* scholarship, Chaunu is perhaps the most productive member of the group. Completing his twelve volumes on Seville and the Atlantic (1504–1650) in 1960, he has devoted much of the past decade to two studies of European expansion for the *Nouvelle Clío* series. He has capped his career at Caen and the Sorbonne with a pair of syntheses, one for the civilization of seventeenth-century Europe (1966) and more recently, one for that of the Enlightenment.

Six years ago Chaunu concentrated upon the apparently paradoxical circumstance of the birth, development, and triumph of a mechanistic cosmology within a self-contained Europe suffering from illiteracy, war, economic crisis, and demographic stagnation. Environmental difficulties notwithstanding, the Age of Reason survived; and as its spokesmen turned from ontology to empiricism in the eighteenth century,

it commenced molding a material civilization in its own image. The present volume underscores the coming to terms of mind and environment in the Enlightenment. From the 1680s on, increasing numbers of Europeans consciously or instinctively ordered their collective existence in a manner consistent with empiricist thought. To Chaunu the fruit of this filtering down of certain ideational processes, this triumph for intelligence, is illustrated in most dramatic fashion demographically. The gain of ten years of life from 1680 to 1780 and the sparing of forty millions, who a century earlier would have perished in infancy or adolescence, derive—according to Chaunu—from the practical concerns of Enlightenment thought, from “le retour des pensées sur les choses.”

In support of his position Chaunu entertains a polemical stance that runs consistently through his volume. Though he avers that the eighteenth century represents the climactic moment of traditional European civilization, the heroes of his hypothetical Enlightenment number those pragmatists who led others into the value system of nineteenth-century liberalism. They include improving English landlords; those enlightened despots and their servants sincere in combating illiteracy and hunger; thinkers such as Locke and Voltaire who constructed political theories upon a conception of the possible; all empiricist scientists and scholars from Leeuwenhoek through Abbé Nollet and the Benedictines of St. Maur; Kant who legitimized an interior faith as he fixed the limits of reason; and Chardin who painted “des choses qui ont une âme, parce qu’elles sont la récompense de l’effort intelligent de l’homme, du travail.” On the other hand, Chaunu’s villains are those who refused to let ontology die, the pure theorists who turned their backs upon the facts or progress. They include French *rentiers* and *parlementaires*; Rousseau and Mably who pushed European political thought toward the utopian, the unrealizable, and the destructive; materialist theoreticians such as Holbach and La Mettrie whose “systématiques grossières trahissent l’esprit [empiriste] des Lumières”; and rococo artists working in a false baroque, a baroque denuded of its supernatural *raison d’être*. Chaunu’s opinions are sometimes outrageous and occasionally amusing. His associating the

Curé Meslier with the Black Mass and the (unproven) French practice of *coïtus interruptus* with the poison of neo-Augustinian pseudoasceticism provoke chuckles, not thought.

Chaunu’s idiosyncrasies notwithstanding, his *Civilisation* has a good deal to recommend it. There are 247 superb photographs with commentary, representing a marriage of illustration to text that brings to mind Lewis Mumford’s *City in History*. Yet the book’s chief merit lies in the summaries on the state of historical quantification in France today. The chapters on population densities, growth rates, migratory habits, and the correlations between literacy and mortality are highly useful. Chaunu accepts uncritically too many of Le Roy Ladurie’s hypotheses on the relationships between climate and social habit, but it is good to see such seminal work obtain the advertising it deserves. The same holds true for the quantitative historical studies of the book and its eighteenth-century readership now being undertaken in France and the United States, though a slip in transcription leads Chaunu to estimate for four years (1723–27) what in reality represents legal French book production for sixty-six years (1723–89). The aforementioned areas, as well as those concerning social attitudes toward marriage, women, children, and death, represent fields where vigorous research in Western Europe and the United States is resulting in an imaginative and carefully constructed monographic literature. Enduring Chaunu’s hypotheses and polemic is not too high a price to pay for being introduced to work of those who are transforming our entire conception of society in the *ancien régime*.

RAYMOND BIRN

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FRANCO VENTURI. *Utopia and Reform in the Enlightenment*. New York: Cambridge University Press. 1971. Pp. v, 160. \$8.50.

Professor Venturi of Turin, widely known as a cosmopolitan, erudite, and judicious writer on the political history of the Enlightenment, has concentrated in *Utopia and Reform* upon two aspects of the subject about which he has been publishing for the last thirty years. He examines the extent to which eighteenth-century republicanism derived less from the exemplars of



classical antiquity than from the recent history and character of Italian, Flemish, and other communes and of Dutch, Swiss, English, German, and Polish state systems. He then isolates the problems of vital importance in the developing social consciousness of the age, problems involved in considering the right to punish. Though discussion focuses upon these two matters, the volume is full of illuminating remarks on the bibliography of the *Aufklärung*, on its motto, "Dare to know," on England's puzzling shortage of philosophes comparable with those of the Continent and Scotland, and on the chronology and geography of the Enlightenment.

In the sixties, though even then tension was growing, the crisis of conscience was over; religious and moral problems had given way to political and social concerns, Pyrrhonism was supplanted by a new faith in nature, philosophic system by experimentation, and legal questions by economic inquiry. Even in remote parts of Europe the encyclopedists of Paris stimulated thought, while slowly revolutionary movements in Corsica, Russia, America, and elsewhere in turn encircled France.

With the French Revolution the cult of the ancient world revived and must be distinguished from the republicanism inherited from the Middle Ages and Renaissance. Montesquieu's scrutiny of the problems facing republics was always in the context of his own times, which forced him to consider size, federation, moral spirit, and internal and external dangers in relation to the threatened contemporary republics. Venice and Genoa, the United Provinces and Switzerland suggested questions. A new republic in America later illustrated one answer to these, as in mid-century the English compromise had offered another.

Venturi connects the right to punish with the fundamental relationship of individual, society, and property. Philosophes confronted by the existence of crime, and the need to repress it, decided that the state's only tasks were to estimate the damage caused by law breaking in both public and private spheres, and to restore harmony. Legislators and jurists, Beccaria declared, should rule "tremblingly" and try to combine equality and freedom with order and security.

A brief description does but scant justice to

the richness and originality of a slim, but difficult and concentrated book.

CAROLINE ROBBINS

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MICHEL DEVÈZE. *L'Europe et le monde à la fin du XVIII<sup>e</sup> siècle*. ("L'évolution de l'humanité," Volume 71.) [Paris:] Éditions Albin Michel. 1970. Pp. 703. 12 fr.

This will be an encouraging book for those who fear that history may disappear in an accumulating mass of microscopic studies of local matters. Michel Devèze, a professor at the University of Reims, has indeed proved himself in the field of intensive research, having produced some years ago two large volumes on the forests of sixteenth-century France. Now he offers us a work of synthesis in the best French tradition of that difficult art. It is a true synthesis, carefully put together, piece by piece, with over eleven hundred references to hundreds of specialized books in half a dozen languages. It reflects the broad views associated with the name of Fernand Braudel and the *École Pratique des Hautes Études*, and it is at the same time volume 71 of the series *Évolution de l'humanité*, founded by Henri Berr over sixty years ago and now carried on by the Centre International de Synthèse.

The author begins with the formation of the concept of Europe in its modern sense in the seventeenth century. This he attributes to a new perception of the globe and the continents that came from the age of discovery, to a differentiation between the old and new worlds, and to a feeling that the peoples of western Eurasia were distinguished, no longer as Christendom, but by sharing in a common secular culture, which had technical and organizational advantages in the production of wealth and power. The superiority of Europe in these respects, while becoming apparent in the fifteenth century, was fully evident in the eighteenth, thanks in part to the decline of Islam and the withdrawal of China and Japan into themselves, but owing mainly to the unprecedented development in Europe of a spirit of innovation. This spirit, which favored science, invention, governmental restructuring, conquest, and expansion, is explained (as by Ranke and others) by the competitive relationship among the European peoples in contrast to the more



peaceable and stable but hence less innovative civilization of China. In any case, by the eighteenth century, and by the use of a marked maritime superiority, both naval and commercial, Europe was the most active and aggressive member of an interconnected worldwide system of which it was also the main if not the only beneficiary.

It is impossible to summarize the rich detail of this enormous canvas. Its main sections present the impact of Europe on Asia, Africa, and the Americas, and then the impact of these other continents on Europe itself. We are not put off with mere generalizations. There are fascinating particulars on a wide array of topics—the Jesuits in Paraguay; the Dutch in Japan; the Russians in Siberia; the production of gold and silver in Spanish America and Brazil; the impact of tea, coffee, cotton, and South Pacific astronomical observations in Europe; the democratic ideas of John Wise in Colonial Massachusetts; and the deism of Ethan Allen in early Vermont. The author's knowledge is so enormous that it seems absurd to point out the few errors that any one reader can identify; nevertheless, his mistaken idea that J. S. Copley and Benjamin West were born in England leaves him with a less than adequate appreciation of the state of the arts in the Anglo-American colonies. His bibliography shows an extraordinary familiarity with books in English. He knows the work of Needham on China, of Curtin on the Atlantic slave trade, and of Bamford on forests and French sea power. His treatment of Japan is derived almost entirely from works in English, including those published in English by Japanese scholars. It is surprising and unfortunate that he has apparently missed a few works that could have been useful to his purpose, including McNeill's *Rise of the West*, Lach's *Asia in the Making of Europe*, and Davis's *Problem of Slavery in Western Culture*.

For so vast and complex a theme no single century offers a very satisfactory basis for periodization. The author finds, however, that the close of the eighteenth century was in its way the end of an era. If Britain won out as a colonial power in the Napoleonic wars, the French won the ideological battle with the beginning of diffusion of ideas of the French Revolution to the rest of the world. The antislavery move-

ment portended the end of the eighteenth-century system. With Abbé Raynal and others doubts were expressed on the racism and exploitation that underlay the great positive accomplishments of the worldwide system of commerce, exploration, and science. The French expedition to Egypt in 1798 stirred Islam, the slaves in Haiti rebelled, and the independence of Latin America could be foreseen. The ultimate limits of the expansion of European power could be faintly discerned—in the West by the independence of the United States and in the East by the resistance to Europe of the sleeping giants, Japan and China.

It is books like this that, for teachers, might help to solve the problem of combining the negative thing called "non-European" history with European history as we have known it.

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DONALD DREW EGBERT. *Social Radicalism and the Arts: Western Europe. A Cultural History from the French Revolution to 1968*. New York: Alfred A. Knopf. 1970. Pp. xxxiii, 821, liii. \$15.00.

Professor Egbert's work on *Socialism and American Life* (1952) led him into a study of modern radicalism "as reflected in theories of art, works of art, and the social activities and beliefs of their creators" from the French Revolution to the present day. The result is, as the author notes in his preface, not art history but the history of "social conceptions as they affect and are affected by the theories, actions and creations of . . . the modern artist." It is also the story of the alienation of avant-garde artists from the state and society. The first part of the book deals with the chief ideologists of social radicalism in the eighteenth and nineteenth centuries; the subsequent three sections cover France since 1815, England, and the rest of Europe. The survey of the esthetic views of the early socialists and anarchists was no doubt the least complicated part of the book to write, and it is also the least controversial. While Marx and Engels are frequently quoted and even more often invoked to this day in this, as in so many other contexts, they did not really aim at providing specific guidelines for art and artists. In the West, at any rate, the direct political impact of utopian socialism and anarchism on the arts was probably larger than that

of Marxism. Saint-Simon's concept of the artist as a leader of society had considerable appeal; Fourier's influence on surrealism (and beyond) and the impact of anarchism on the symbolists and the New Left is well known. There are some curious lacunae: Lassalle is mentioned, but only in passing; Franz Mehring's name does not even appear; nor does the Gotha party conference of 1896 figure, the first occasion, I believe, in modern times, on which the attitude of the proletariat toward modern art was discussed by the highest body of a Socialist party with millions of followers. Generally speaking, Professor Egbert devotes far more space to developments in France and Britain (and seems more familiar with them) than to trends in Central and Southern Europe. There is a highly informative chapter on Belgium, but Austro-Marxism and its attitude to modern art is not even mentioned. The same applies, incidentally, to the narrower field of art history; the reader will look in vain for a discussion of Avenarius and his circle, for Max Liebermann and the Berlin *Sezession*.

But the real difficulties in a work of this kind lie beyond the discussion of esthetic theories. The author does not restrict himself to the fine arts and the "minor arts" but frequently refers to the theater and the cinema and to literature and music; this is a sensible approach because it is clearly impossible to deal with the "fine arts" in isolation. But once the scope of the investigation is broadened, the choice of illustrations and references becomes by necessity selective and arbitrary.

It is of course well known that the political and the artistic avant-garde have by no means been identical, and the discrepancy between the two has, if anything, become more pronounced over the last seventy years. It is comparatively easy to point to attempts to change the environment made by artists and critics from Ruskin and William Morris on; it is far more difficult to show, except in a very general way, to what extent artists have been determined in modern times by their environment. Art forms and, a fortiori, personal style have to a large extent developed independent of the social order, and there is no obvious connection between revolution in the arts and political revolution. To give but one example: the founding fathers of nonobjective art, Kandinsky and Kupka,

Klee and Feininger, were all radicals of sorts, but what Professor Egbert says about Feininger applies to all of them: "his social interests scarcely affected his art." The same, needless to say, goes for Schönberg and Stravinsky, for Proust and Kafka and Joyce. Only a few artists have opted for total political commitment (for example, Fougeron, Taslitsky, and Paul Hagarth), but they were not exactly the greatest of their age, and the political tribulations of Picasso are well known. The author makes the interesting point that the choice of the medium matters in this context. The most famous radicals such as Daumier, Steinlen, and Grosz were all essentially graphic artists; when they turned to painting their art was not of the same quality.

Professor Egbert's book is a mine of information, and it will offer something of interest to every reader. His judgment on major issues is sound, and the conclusions that emerge are unstartling: that many artists were to a varying degree alienated from society; that being individualists, they tended toward some form of libertarian anarchism rather than collectivism and strict party discipline; or that artists are more likely to be radical in their youth than in their old age. The author notes that in recent decades the alienated avant-garde has become in the noncommunist world the artistic mainstream with the active help of the establishment; as a result there is no longer an avant-garde.

If on concluding this massive volume one feels nevertheless a sense of dissatisfaction, this has to do to some extent with the intangible and inchoate character of the subject: to deal adequately with the many topics raised, a dozen volumes would be needed. At the same time the case for studying in detail the artist's political ties is not proven; if the political orientation of the artist, as Egbert says, does not account in the deepest sense for the artistic merits of his work, they cannot possibly provide a major key to the understanding of his work. The class interests and the political aims of the peasantry are obvious, those of the artists are not. The answer provided by Herbert Read (that there has never been before the modern epoch an art without social significance) is not quite convincing, for "social" is not a synonym for "political." True, the artists'

politics are of sociological interests, but then a third-rate artist may well reflect the *Zeitgeist* more accurately than the lone, isolated genius. Among the shortcomings of the study is the fact that it reads in parts like a catalog of artists and their work, which does not always clarify their respective importance or relevance. The ideological impact of the writings of Francis Klingender and Christopher Caudwell is surely not equal to that of the leading Continental left-wing theoreticians in this field. The terms "radical" and "Marxist" are used rather freely and sometimes indiscriminately; to call Wilhelm Hausenstein or Walter Benjamin Marxist is at least questionable. William Morris is alternatively called a "Marxist" and a "revisionist"; but even the revisionists did not reject, as Morris did, the class struggle. On Romain Rolland it is said that he "died a convinced communist although he had never entirely shed the influence of East Indian mysticism," which means of course that, with all his admiration for Stalin, Rolland was not a Marxist. Again, the fault is not entirely the author's, for artists have been (and are) notoriously inconsistent in their ideological beliefs. But this makes it all the more important to differentiate between Marxist and *Marxisant*, between Communist and *Salon Communist*, between socialist and antiestablishment attitudes—or to refrain from such classification unless it can be done with a minimum of confusion.

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JONATHAN BEECHER and RICHARD BIENVENU, translated, edited, and with an introduction by. *The Utopian Vision of Charles Fourier: Selected Texts on Work, Love, and Passionate Attraction*. Boston: Beacon Press. 1971. Pp. xiv, 427. \$12.50.

This volume, introduction, selection, translation, and notes, represents an excellent piece of work. The introduction provides a penetrating summary of Fourier's remarkable system. It both emphasizes its essential unity and treats with sympathetic understanding its diverse parts. The selections are generally brief, very numerous, and skillfully arranged to present with great richness and fidelity the strange teaching of the father of the phalanx. The exposition moves from "first proclamations" to

"commerce, industry, and work in Civilization," and "philosophy, morality, and sex in Civilization," followed by "the theory of passionate attraction," "the ideal community," "attractive work," and "the new amorous world," with "the mathematical poem," Fourier's theories of cosmology and analogy, as the conclusion. The book also contains a preface, a note on the sources, a bibliographical note, a glossary, and an index. The translation is splendid, a feat that can be properly appreciated only by those who have worked much with Fourier. The notes, that is, brief introductions to the several sections, as well as some more technical notes at the bottom of the pages, constitute the least distinguished part of the volume; but they, too, serve their purpose in helping the reader.

Some particulars, of course, can be criticized. There are, for example, the rather lame pages on whether Fourier's system was or was not tyrannical (pp. 271-73), whereas Professors Beecher and Bienvenu really know that everything depended on the efficacy of Fourier's formula: if the formula is valid, life in Harmony would be the very essence of rapturous liberty and joy, if it is not valid, there would be no Harmony, or, if something of the sort could be somehow imposed, it would be a total tyranny. Again, while the authors are completely correct in emphasizing the extreme refinement and good taste of the future society, they are nevertheless wrong when they assert that "Harmony would have no room for the Promethean dynamism or the sweating bodies and calloused hands" (p. 72). As Fourier repeatedly informed us, the marvels of Harmony would include just such dynamism, found only very occasionally in present-day life, for example, when miners dig desperately to rescue trapped comrades or when soldiers storm enemy positions in the heat of battle. But these and other possible cavils do not impair the generally very high quality of the volume.

It is good to have so much Fourier so well presented and readily available in English. Although the prophet of Harmony aimed to encompass the universe in a formula, he also had formulas for everything else, some of them a paragraph or a sentence long, and these formulas too often stop the reader. "The poor incessantly attempt to rob the rich on an individual

basis, and the rich continually plunder the poor as a class" (p. 302).

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ORON J. HALE. *The Great Illusion, 1900-1914*. (The Rise of Modern Europe: A Survey of European History in Its Political, Economic, and Cultural Aspects from the End of the Middle Ages to the Present.) New York: Harper and Row. 1971. Pp. xv, 361. \$8.95.

Two ideas, theses almost, link the many details of this book. One is that "the years from 1900 to 1914 were not simply the sunset of the nineteenth century—or the last act in a Victorian play—but rather a period that clearly belongs to the twentieth century," not an end but a beginning, "not the twilight of a golden age but the seedbed of our twentieth century problems and concerns." The other thesis is that while it was the great illusion of a progress-proud age to believe that a general war among the nations of Europe had become unthinkable, that war, when it came, was by no means inevitable, that "it cannot be argued convincingly that the First World War was predestined or prefigured in the arts and sciences, in economic relations, in the area of ideas, or even in the public relations of the peoples of Europe."

Case proven? Not quite, not on the first count, that is. There are of course some formidable obstacles in the way of showing that any given fourteen years will form a historic unit, even if they happen to occur between the opening of a century and the beginning of a war. Then, too, the nature of the book, designed as it is as a survey in the Langer series, makes the task no easier. The mass of detail tends to obscure whatever pattern may exist—the eternally carping critic: too many details on the one hand, too few on the other (sometimes, the urge to inclusiveness results in an almost private shorthand). Or will the reader really know, without further explanation, what either plural voting in Belgium or the Selden Patent were, or be able to make very much of a half-line mention of De Mun? There is the fact, besides, that on more than one occasion the discussion of early twentieth-century trends reaches back into the nineteenth, and rightly so, since the discussion of new departures in bi-

ology would be as incomplete without Darwin as the description of the automobile industry without Daimler.

On the second point, that the war was forecast neither in the stars nor in the hearts of villainous politicians, the book is very much more convincing. One reason is that the narrative finally slows down here; there is a chance for breath and for reflection. The other is the profound understanding not only of European diplomacy but of the European mood that suffuses these pages. Thus there are, to balance the obligatory accounts of friction, some excellent passages on the many equally real efforts at cooperation and accommodation. The Balkan Wars and the Moroccan crises are fully described, but so is the formation, in the same period, of several hundred international organizations, governmental and private. Thus, while the book is clearly a post-Fischer one, the judgments are full of the sort of old-fashioned good sense that may well be tomorrow's style again—on the determinist road to war pattern: "wisdom made manifest after the event"; on Bethmann Hollweg: "There were flaws and weaknesses in his makeup, and he woefully misjudged the situation in 1914. But fundamentally, he was a man of honor, sensibility, and peace." Was the writer unfamiliar, then, with the past decade's torrent of argument? No, he was fully aware "that current research on German war aims has revealed a new Bethmann Hollweg, but it should be remembered that war leadership produces unique transformations; the Lloyd George of 1909, for example, was not identical with the Lloyd George of 1918."

There may, of course, be those who will disagree still and side with the revisionists of the 1960s. But after Hale, they had better be very sure of their ground. And, in general, there is something fairly churlish about any criticism of this volume, the preceding lines included. For in essence this is a book by an author who has read widely and well, who has taken note of all recent works of importance and of much unfamiliar material as well (there cannot be many other American historians of Europe who refer quite so frequently and casually to the *Neue Zürcher Zeitung*), who has kept his own good counsel throughout it, and who has provided the reader with a path through a

thicket of facts. It would seem quite enough for one book.

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PAUL G. HALPERN. *The Mediterranean Naval Situation, 1908-1914*. (Harvard Historical Studies, Volume 81.) Cambridge, Mass.: Harvard University Press. 1971. Pp. x, 415. \$13.50.

On the eve of the First World War many European naval officers expected to see, when the struggle began, a great and decisive battle between England and Germany in the North Sea. There the main protagonists faced each other, the hostile fleets separated by a mere twelve hours steaming time; and there, at Jutland, the only large naval battle of the war did take place. As a result most research on the navies of the period has centered in the gray Atlantic. But what of the blue Mediterranean? The French, English, Italians, and Austrians, not to mention the Russians and Turks, had strategic interests and bold dreams for this area.

Mr. Halpern's book is the first comprehensive study of the prewar naval situation in the Mediterranean. He has obviously spent a number of years on the work, and it is a definitive account. The author has consulted the archives of France, Britain, Italy, and Austria as well as many private papers and relevant individuals. The book devotes a chapter to each of the main powers: France, as the dominant nation; her chief rival, the Italians; and the Austrians, whose navy grew from a coast defense force to a major contender in the Mediterranean balance of power. The fears and hopes of that by-gone era are evoked. France was apprehensive about the possible union of the Italian and Austrian navies, which would seriously threaten her commanding position. She could count on little help from the British, who had withdrawn the bulk of their ships to the north. The Italians, with their technically daring and beautiful vessels, saw a chance—with their old antagonists, the Austrians, now allies—of sweeping the French from the sea. And the Austrians, seeking a wider role, spent lavishly for enormous dreadnoughts that they really did not need for their defense. These great battle-ships were then a symbol of prestige, and na-

tions from Chile to Turkey wanted them, whatever the cost.

This was a period of innovation in ships and innovators in naval administration: Fisher and Churchill in England, Delcassé in France, Grigorovitch in Russia, Montecuccoli in Austria, and Cattolica in Italy. They purged the deadwood in the officer corps, kept their ships at sea and away from ceremonial functions, and generally tightened up the slack conditions that had crept in during the long years of peace.

The last chapter carries the drama forward to its conclusion and shows the actual events that occurred in wartime. It is, as the author states, an anticlimax. Italy remained neutral and later joined the Allies, thus ending the French fear of an Italian-Austrian combination against them. While Austrian submarines were active, no major action was fought by that navy.

The book has no photographs—a pity, since the Imperial War Museum has such a fine naval collection for this period. The author could have spiced the story with a few anecdotes as his writing is sometimes rather unvaried. But he handles the mass of detail as well as the dominant themes in a straightforward manner. For those who wish to know the period, it is an indispensable book.

REGIS A. COURTEMANCHE  
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ALBERT SEATON. *The Russo-German War 1941-45*. New York: Praeger Publishers. 1971. Pp. xix, 628. \$15.00.

Thirty-one years after Hitler's invasion of the Soviet Union, the Russo-German war continues to attract the attention of historians, military writers, and journalists. There are, still, some major problems connected with this aspect of World War II that, in its duration and ferocity, was the counterpart to the Allies' war against Japan. Why did Hitler start this campaign that proved so costly for Germany? Why, in spite of repeated warnings, was Stalin caught off guard in the beginning? Why did the fortunes of war change so dramatically? These are not easy questions, and Colonel Seaton has tried to answer them in a scholarly and readable book, which should please the specialist as well as the general reader. Based on a



broad range of German sources—official documents, journals, war diaries, unit histories, and personal diaries and memoirs—and on the available official Soviet histories and memoirs, the author concentrates on military operations and on problems of leadership and command responsibilities. The relative paucity and inaccessibility of Russian material is reflected in a somewhat unbalanced treatment, which, except in the case of war atrocities, favors the German side in breadth and detail. The summaries of strategic and tactical decisions and the assessment of responsibilities of military and political leaders on both sides are the most valuable parts of this study. The descriptions of military operations, on the other hand, are involved and complicated and not easy to follow, and although there are maps for each major campaign, they are, for the most part, poorly drawn and of little help.

Discussing the pre-1939 European diplomatic developments, Seaton believes "that Chamberlain was right and Churchill was wrong" in their assessments of the Soviet Union, which, in Seaton's opinion, was not ready to come to the aid of Czechoslovakia. He blames Hitler for starting the war in the east and for the German defeat at Stalingrad. Germany had no chance of winning the war to begin with, because Hitler was confused in his military and political aims, his intelligence service was poor, his resources were insufficient, and his general staff was inadequate. And while Stalin and the Soviet High Command made serious mistakes throughout the war, they were, on balance, superior to Hitler and his generals. In the last analysis, however, "Britain and the Soviet Union owed their survival to geography and to climate and only secondarily to their own endeavours."

In closing and with unacknowledged hindsight, Colonel Seaton faults Britain and the United States for not invading Europe through the Balkans and Scandinavia to defeat Hitler and, at the same time, to prevent the Soviet Union from dominating Europe.

GEORGE O. KENT  
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KARL HNILICKA. *Das Ende auf dem Balkan 1944/45: Die militärische Räumung Jugoslawiens*

*durch die deutsche Wehrmacht.* (Studien und Dokumente zur Geschichte des Zweiten Weltkrieges, Number 13.) Göttingen: Musterschmidt. 1970. Pp. 404. DM 78.

More than half this volume is taken up by reprints of documents; the rest is an illuminating study of the last pathetic phase of Hitler's venture into the Balkans. The author's declared purpose is to set the record straight because both the 120-volume Yugoslav official documentary history of the war and the two-volume work by the Yugoslav general staff are "one-sided, have pronounced biases, and contain falsifications and exaggerations" (p. 9).

In his effort to present an objective account Hnilicka has tried to study all the documentation everywhere, and he deplores the fact that while the papers in American and British archives are readily available those in Yugoslavia and the Soviet Union are not. He makes good use of what he had access to and has indeed come up with a lot of raw material. He perused the published memoirs of participants and conducted interviews with survivors both in Germany and outside, notably in Yugoslavia itself.

The title is somewhat misleading, for the work does not deal exclusively with military affairs. Perhaps its most interesting and valuable sections, however, are the accounts of the military, SS, secret police, and Nazi political agents who were attached to the Hitlerite satellites in the Balkans—Nedić's Serbian and Pavelić's Croatian regimes. The author shows great understanding of the latter. Indeed, he declines to consider General Nedić a collaborator in the customary sense of the word so much as "an honorable Serbian royalist" (p. 47)—although it would have been helpful had he defined exactly what he meant by the customary sense of the word.

The author is concerned with a topic the essentials of which have been obscured by the partisan versions of history written by the victorious forces of Balkan communism. He is to be commended simply for attempting to tell the other side of the story in a scholarly fashion. What does become clear from his book is that had the peoples of the Balkans had their wish, the situation in the area would be very different from what it is now, for the noncommunist population had looked to be liberated



by British and American troops rather than the Red Army (p. 48).

Hnilicka's book makes a substantial contribution to the military, international, and domestic history of the last two years of World War II in the Balkans. Any work of scholarly standard that assails the monolithic interpretation of the history of any period is to be welcomed. So is the present one.

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MAY MCKISACK. *Medieval History in the Tudor Age*. New York: Oxford University Press, 1971. Pp. 180. \$7.25.

The search for British antiquities in the reign of Henry VIII was primarily motivated by the necessity for justifying the Reformation in the English Church; by the time of the founding of the Society of Antiquaries in 1586, the Tudor establishment having long since been reaffirmed by the queen, antiquarianism appears often to have become an end in itself. Thus, Bishop Godwin in 1601 confesses that his delight in the study of history and antiquities "hath been somewhat greater than was needful for a man that had dedicated himself and his labors unto the service of God's church in the ministry." Oblivion had long since become as potent an enemy as the pope. Miss McKisack tells the story of this antiquarian urge beginning with Leland's itineraries with license to salvage books from the dissolved monastic libraries, through the more coordinated efforts of Parker and Cecil to recover and print early British historians both ecclesiastical and secular, to the patriotically oriented topographers Stow and Camden. Even the queen, on the occasion of receiving Lambarde's digest of the records in the Rolls Chapel, responded that she would be "a scholar in her age."

Miss McKisack gives generous acknowledgment to earlier studies of the movement, notably that of T. D. Kendrick (1950), but she has resurveyed the field with manifest profit, updating the scholarship and making new and extensive use of manuscript materials in the royal, diocesan, and university collections. It is therefore the more regrettable that she came so late upon F. J. Levy's *Tudor Historical*

*Thought* (1967), which had already dealt with many of the antiquarians she treats. Her chapter on general histories of Britain is the least satisfying since it is relatively derivative and, as a result, occasionally inaccurate. C. L. Kingsford's *English Historical Literature of the Fifteenth Century* (1913) still remains the soundest authority here. Value judgments are for the most part avoided. But to say that Protestant prejudice is a "major handicap" of Elizabethan historians is not merely to ignore the equally prejudiced Catholic historians of the period but to overlook the historical value of the prejudice itself.

W. GORDON ZEEVELD  
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CONRAD RUSSELL. *The Crisis of Parliaments: English History, 1509-1660*. (The Short Oxford History of the Modern World.) New York: Oxford University Press, 1971. Pp. xvi, 434. Cloth \$9.75, paper \$2.95.

This survey of English history from the accession of Henry VIII to the Restoration of Charles II incorporates the results of much recent research, but it is not merely a compendium of other historians' views. It is an intelligently constructed synthesis that is reinforced by the author's own research and enlivened by his selection of appropriate and often humorous examples and quotations. A sensible balance between factual and illustrative material makes this book more readable than many studies of its kind. Although it takes the form of a political narrative, it does not suffer from a lack of thematic emphasis or analytical insight. An introductory portrait of Tudor England and a chapter entitled "Puritanism and Fashion 1570-1640" include a variety of topics that can be discussed only within a broad chronological framework.

Although Mr. Russell does not give sufficient coverage to humanism and the problem of poverty, he describes other social, economic, and intellectual developments adequately. His discussion of the related issues of inflation and government finance merits specific praise. Russell is most successful, however, as a political historian. He offers an excellent account of the parliaments of the early seventeenth century and the best brief summary of the ship-money dispute to date. In analyzing religious develop-

ments he engages in a current debate by arguing that Puritanism, even in its Presbyterian form, was essentially conservative. It became dangerous to the Crown only when it moved into political opposition and revolutionary when fused with the secular radicalism of men like John Lilburne.

The central problem of explaining the civil war and revolution of the mid-seventeenth century Russell approaches cautiously. He argues that the war itself was unintentional and is therefore explainable only by mutual fear and distrust. The collapse of the system of government that preceded the outbreak of war he attributes to the failure of the government's religious and financial policies. He tends to be suspicious of underlying social explanations, insisting quite correctly that the revolution in its first stages did not constitute a challenge to the established social order. Yet this should not have prevented him from clearly relating the political breakdown of the early 1640s to the social changes of the preceding one hundred years. Although the author establishes a connection between the educational revolution and the growing self-confidence of the gentry in the House of Commons, he does not give proper attention to the crisis of the aristocracy or the alienation of the "country" from the "court." He is reluctant to take a stand on the gentry controversy, which he considers to be a statistically insoluble problem of rising or falling incomes. It might have been more fruitful to view the "rise of the gentry" as a complex social problem concerning the changing relationship between the gentry and the peerage rather than a strictly economic process.

Nevertheless Russell's book, which is part of an important series, deserves a wide audience. A reliable and up-to-date introduction for the student, it will serve the historian as a careful, balanced, and lively synthesis.

BRIAN P. LEVACK  
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J. D. GOULD. *The Great Debasement: Currency and the Economy in Mid-Tudor England*. New York: Oxford University Press. 1970. Pp. viii, 198. \$7.00.

Professor Gould has written an almost model monograph on the highly complex, technical,

and easily misunderstood subject of the Great Debasement. He leads both general Tudor and expert economic historians alike gently, but with evident enthusiasm, through the labyrinths of Tudor minting of coins and government monetary policy and resolves a number of scholarly controversies along the way.

He specifies the scope and limitations of his book at the outset: that his is the first study of the subject by a modern economic historian but that it lays no claim to be a "balanced and systematic view of all economic aspects of the Great Debasement" (p. 5). When one considers the thoroughness of this volume, however, one cannot help wondering what the author has omitted.

Other scholars will have to test the broadest significance of this study claimed by the author—that "the theory of mint affairs and of the foreign exchanges offered in this work . . . should . . . prove useful to economic historians who wish to concern themselves with monetary and allied topics in any part of the medieval and early modern periods" (p. 6). They will require a sufficient body of data such as Professor Gould had at his disposal (and he is admirably cautious in explaining the evidence he has used, its limitations, reliability, and the gaps in it). Evidently modern quantitative techniques and analytical tools may have limited applicability if, for example, the author is correct that no adequate assessment of imports and therefore no clear balance of payments position can ever be established. He pleads, moreover, for the creation of an import index, for which the essential requirement will evidently not be a knowledge of econometrics, but the kind of ingenuity in using scattered, difficult-to-interpret evidence that has always characterized the best in medieval and early modern historical scholarship.

The text is admirably printed, with a few very minor errors. Where feasible, charts have been included near the text they illustrate. Only a few extensive tables and three graphs have been relegated to the end of the volume. A few topics are discussed in appendixes, several shorter technical points in explanatory footnotes.

Professor Gould may chiefly be faulted for a tendency to protest too much his inadequacy to deal with some aspects of his subject. While in-

tellectual modesty is as commendable as it is rare, no one presupposes the historian's omniscience (and the author pays full tribute to those upon whose work he has built, while he is very generous toward scholars with whom he disagrees).

He uses rather haphazardly "I," "we," and "one." For my taste the "we" might well be eliminated altogether since the author is neither king nor pope.

One can only trust that Professor Gould's remoteness from manuscripts in England will not prevent his making further significant contributions to the study of this period. One would anticipate reading them with both profit and pleasure.

HOWARD S. REINMUTH, JR.  
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WILLIAM S. MALTBY. *The Black Legend in England: The Development of Anti-Spanish Sentiment, 1558-1660*. Durham: Duke University Press. 1971. Pp. 180. \$6.75.

At the beginning of this century, Julian Juderías, a Spaniard disturbed by his country's reputation for cultural backwardness, coined the expression "Black Legend" to refer to the writings of Spain's critics past and present. Soon after, historians picked up the term, using it primarily to describe the literature concerning Spanish maltreatment of the Indians. Now William Maltby, investigating anti-Hispanism in Renaissance England, applies the term to Tudor-Stuart works on Spain, including translations of sixteenth-century Spanish histories, religious writings of early English Protestants, literature on the revolt of the Netherlands, overseas expansion, and the Armada, as well as other miscellaneous pamphlets produced in the seventeenth century. Maltby examines these works one by one, shows how they consistently exaggerated, distorted, and misrepresented Spanish actions, and claims that they were responsible for establishing an English stereotype of the Spaniard as cruel, immoral, treacherous, and proud. This stereotype, according to the author, was reinforced by similar strains of anti-Hispanism in France and the Netherlands, so that the "legend of Spanish barbarism was allowed to grow and to become part of the intellectual baggage of Western man."

Black Legend is a loaded term: and Maltby's

decision to adopt not only the term but also Juderías's thesis that there has been some sort of virulent anti-Hispanism present in the last four centuries of Western thought was an unfortunate one. It led him to oversimplify Tudor-Stuart conceptions about the Spaniard and to ignore the real ambivalence in the Englishman's attitude toward his enemy. Renaissance Englishmen may well have hated the Spaniards, but, in writers such as Raleigh, there is also a certain awe, which sometimes even borders on admiration, for their power and accomplishments, particularly in the New World.

Maltby's preoccupation with the Black-Legend thesis also impels him to devote an inordinate amount of space to exposing the biases and inaccuracies of Tudor-Stuart writers without bothering to develop in any systematic or meaningful way the nature of the relationship he admits exists between these biases on the one hand and the Reformation and incipient nationalism on the other. For example, his book contains no discussion of the recent scholarship on Tudor-Stuart millenarianism, even though it is obviously pertinent to his subject. As a result, his study adds little to what is already known about the ideology behind the Protestant nationalism of England in this period.

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K. R. WARK. *Elizabethan Recusancy in Cheshire*. (Remains Historical and Literary Connected with the Palatine Counties of Lancaster and Chester, Volume 19, Third Series.) Manchester: [Manchester University Press] for the Chetham Society. 1971. Pp. viii, 200. £3.60.

This is a monograph of superb scholarship and painstaking research, judicious, and, on the whole, well balanced in its analysis and conclusions. K. R. Wark has added another important study to the growing literature on recusancy in Elizabethan and early Stuart times, providing an in-depth survey of the sources for Cheshire, thus completing the work of T. S. Willan, who surveyed recusancy in Lancashire in the reign of Elizabeth I some twenty-five years ago. Wark continues for yet another county the able work done by Dom Hugh Aveling for Yorkshire in recent years. If such stud-

ies could be forthcoming for all the counties, a much needed major investigation of at least this area of Catholicism in Tudor and Stuart England might be undertaken.

The study begins in 1559 with the enactment of the Elizabethan religious settlement and traces the activities and fortunes of recusant Catholics in Cheshire up to 1603, as well as the efforts of authorities to curb it. Utilizing a wide array of materials—including the most important single source for understanding Cheshire recusancy before 1580, the Metropolitan Visitation Book of 1578, as well as subsequent Diocesan Visitation records, Parish Registers, Bishops' Transcripts, Recusant Rolls, Plea Rolls, Crown Books, Quarter Sessions' Records, Sessions' Depositions and Examinations, Mayors' Letters, and others—Mr. Wark has assessed statistically and through personal vignettes the strength or weakness of recusancy as a serious governmental problem in Elizabethan times. The author succeeds admirably in this while still providing occasional glimpses of the human drama, the excitement of prison escapes, and the poignancy of young boys being whisked out of the country for years of exile and forbidden education abroad (pp. 81, 103, 108–10).

Mr. Wark offers excellent testimony to the impact of the gentry as the largest sustaining group, while noting that the lack of clear leadership from great families whose influence was predominant in the country was decisive in limiting the extent of recusancy in Cheshire, which Mr. Wark puts at a "known" 302 out of an estimated population of 63,865! More interesting is his examination of those social classes least studied in this matter, that is, business, professional or craftsmen, husbandmen, laborers, and servants. As would be expected, the largest number of known recusants were women. This he found to be true even though he indicates that before the 1590s the importance of women went largely unrecognized, so that until that date they were not indicted to the extent that the men were. Mr. Wark also supports with clear evidence the accepted view that as the reign progressed Elizabeth's government resorted less to imprisonment and more to enforcing the laws for severe fines and loss of lands and goods, hoping to impoverish the

recusants into obedience to the laws as a more successful means of obliterating the group.

While Mr. Wark offers little in the way of startling or new ideas, his meticulous research helps immeasurably to substantiate earlier general interpretations. There is, however, a curious inconsistency in his abrupt, and from his own evidence unproven, conclusion that before the end of the reign of Elizabeth I recusancy had developed a vitality no penal law could crush. All his evidence and intermediate analysis had pointed to recusancy as a minor, insignificant problem in the body politic as far as Cheshire was concerned.

The study is invaluable for its lengthy appendix containing a list of Cheshire recusants with a brief summary of what is known about each (pp. 138–73) and in his excellent and thoroughly up-dated bibliography. No scholar in the field can afford to ignore it.

SISTER JOSEPH DAMIEN HANLON  
*St. Joseph's College, Brooklyn*

RICHARD L. HILLS. *Power in the Industrial Revolution*. [Manchester:] Manchester University Press. 1970. Pp. ix, 274. £4.00.

HAROLD CATLING. *The Spinning Mule*. (David and Charles Library of Textile History.) Newton Abbot: David and Charles. 1970. Pp. 207. £2.75.

J. GERAINT JENKINS. *The Welsh Woollen Industry*. Cardiff: National Museum of Wales, Welsh Folk Museum. 1969. Pp. xviii, 410. £2.75.

KENNETH G. PONTING. *The Woollen Industry of South-west England: An Industrial, Economic and Technical Survey*. (Origins of Industry.) New York: Augustus M. Kelley. 1971. Pp. x, 214. \$20.00.

These four volumes reflect the sustained interest of English historians in the British textile industry at a time when it acted as a major catalyst of economic and technological change. The first two studies in the group are confined to technological advance, mainly in the cotton industry. That by Dr. Hills now becomes the best single account that we have of the development of cotton manufacturing equipment between the 1730s and 1820. The author offers no widesweeping reinterpretation of earlier work by Miss Julia Mann and Mr. Walter English. Rather, in a well-documented monograph he probes into the details of technical develop-

ment and especially relates manufacturing equipment to power sources.

Particularly enlightening is his investigation of the work of the inventors John Wyatt and Lewis Paul. Although they partially or fully conceptualized several advanced spinning devices (for example, their intention of placing drafting rollers inside a revolving tube implied tube twisting), it is most unlikely that they ever spun by roller drafting, the major innovation sometimes attributed to them. Mainly because, as Dr. Hills observes, they failed to establish the relationship between fiber length and roller spacing. Besides demonstrating the well-known fact that waterframes and mules, in exhausting traditional power supplies, stimulated the development of the steam engine as a source of regularly transmitted power, Dr. Hills carefully plots the installation of Boulton and Watt engines in textile mills before 1800 and reconstructs the problems facing early engine builders and their textile mill clients. The study seems most impaired by Dr. Hills's neglect of much of the artifactual evidence, the early machinery that may be found in museums in Lancashire and London.

Dr. Catling's examination of the development of the spinning mule is a fascinating, though unavoidably technical, history of a single highly significant machine. In it the author nicely utilizes his rare combination of experiences as a teenage mule piecer and as a long-time textile research engineer. Several aspects of the book struck me as of particular value: the lucid explanations, supported by helpful diagrams, of the techniques and advantages of intermittent spinning on spindle wheel, jenny, and mule; the equally clear accounts of the mule's working parts and their modifications; the results of engineering research into Richard Roberts's quadrant, which the author concludes could achieve "almost perfect winding" (p. 82); the subtle distinctions between the Bolton fine-yarn mule and the Oldham coarse mule and the more familiar differences between cotton and woolen mules; and the author's own recollections and analysis of mule room operations. Inexplicably Dr. Catling has omitted the spinning-mule data found in the "Manufacture of Cotton" article in Rees's *Cyclopaedia*. Though Dr. Catling gives some figures of mule spindleage and output, the eco-

nomic aspects of his account would have come into sharper focus had he used more machine-makers' records (for example, Dobson and Barlow's order books, which run complete from 1851 to 1951). Despite these weaknesses, no one attempting to write about the British nineteenth-century cotton industry should omit reading this book for insight into the technological tastes and limitations of British cotton-mule spinning; for instance, standardized maintenance, possible with interchangeable parts, was frustrated in Lancashire mule spinning because each "gaffer" was allowed to tune his pair of mules to a high degree of individuality.

The two remaining volumes are regional histories of the woolen industry. In his definitive portrayal of that in Wales, Mr. Jenkins examines woolen manufacturing techniques and the industry's pre-sixteenth-century structure, based on home and fulling mill, before treating five manufacturing subregions in turn: Montgomery, Merioneth, north Wales, west Wales, and the southeastern counties. The picture emerges of an industry handicapped by undercapitalization, bad organization, small-scale production, strong conservatism, and a variety of depressing English influences. These last began when the Shrewsbury drapers monopolized the finishing and marketing of Montgomery and Merioneth cloths from 1562 until the 1770s. Later, concentration following factory organization was frustrated by east-west canal and railway networks that severed mid-Wales from Glamorgan's growing industrial valleys and opened central Wales to mass-produced English goods. Merioneth mills survived by processing local wool to meet local needs, until wool prices rose during the Boer War and Welsh farmers turned to English manufacturers for higher profits. But the real problem was the absence of a strong capitalist class, rather than nefarious English practices. In west Wales, the industry's sixty prosperous years ended after the First World War because of failure to modernize equipment or to follow wider fashion trends toward lighter fabrics; dependence on the south Wales workers' demand for medium to heavy woollens had disastrous results during the miners' strikes of 1921-26. Mr. Jenkins's organization of his material leads to some repetition; otherwise only



minor errors were disturbing. The carding machine illustrated on page 36, for example, is certainly not Paul's of 1748; and the water-frame never spun wool, as opposed to worsted (p. 238).

Mr. Ponting's new introduction to the West of England woolen industry consists of a historical narrative, illustrations of extant evidence and artifacts, a collection of documents, a list of surviving buildings, a glossary of trade terms, a bibliography, and an index. I consider the concept of such an integrated survey excellent but wish that the imperfections in its execution were less numerous. The historical account is unbalanced since over a third is devoted to the conflict in 1802-03 between the shearmen and clothiers over the introduction of machinery, a struggle adequately depicted previously by J. L. and Barbara Hammond and Professor Arthur Aspinall. Curiously, the earlier battles over the introduction of the jenny and the longstanding hostility between manufacturing and agricultural interests are hardly noticed. Second, there are too many slips for it to be regarded as a wholly reliable introduction; for example, mistakes in patent dates on pages 47, 48, 55, and 66. Third, the documents suffer from irregular editing; for instance, commentary appears sometimes at the head and sometimes at the foot of the gobbet (pp. 137, 139). Also two important sources have been omitted: the 1661 description of clothmaking by the economist Sir William Petty, son of a Hampshire clothier and the 1823 description by William Partridge, a Gloucestershire dyer who emigrated to the United States. Finally, the American price of this book is outrageous.

In conclusion, three specific defects found in more than one of these studies seem worth mentioning. First, it has long since been shown that neither Leonardo nor Jorgen invented flyer spinning (cf. Catling, p. 14, Ponting, pp. 30-31). Second, B. D. Jackson's invaluable dating of the Rees *Cyclopaedia* articles, the unrivalled source for early nineteenth-century British technology, seems unknown to several authors (Hills, p. 245, Jenkins, p. 59). Last, the omission of footnotes by Dr. Catling and Mr. Ponting exemplifies a deplorable economy increasingly practiced by the newer English presses.

DAVID J. JEREMY  
Merrimack Valley Textile Museum

GEORGE RUDÉ. *Hanoverian London, 1714-1808*. Berkeley and Los Angeles: University of California Press. 1971. Pp. xvi, 271. \$8.95.

This is the first volume to appear in the new History of London series, whose promise of high scholarly character is attested to by the distinguished list of authors that its general editor, Francis Sheppard, has gathered together. Professor Rudé has written a sound, traditional, satisfying history, which places London in the context of broader national developments without ever letting it lose its identity in the process. Chapters on its physical expansion, economy, social structure, amusements, religious life, and government provide lucid and convenient summaries of the standard works on the subjects, but do not attempt to reassess their conclusions.

How differently would Rudé have written the book a quarter century ago? It is in no sense a reproach to him, but certainly one to the industry of London historians, to be forced to answer, not much. The picture of Georgian London given us by Sir John Summerson, Dorothy George, and even the Webbs remains remarkably intact. For its political history, thanks to more recent works by Dame Lucy Sutherland, Ian Christie, Francis Sheppard, and Rudé himself among others, the past few years have seen more fundamental changes in our understanding. The chapters on London politics, therefore, particularly insofar as they deal with the role of the "mob," are the most valuable in the book. But the apparent durability of the earlier studies in the social, institutional, and architectural history of the metropolis, while tributes to their sound scholarship and persuasive interpretations, suggests that the present generation of historians is neglecting eighteenth-century London.

Such neglect is hard to understand; London's libraries and archives abound in unexploited source material, while Georgian London ought to exert a peculiar fascination today: a city sizable even by our standards, precociously afflicted with every conceivable twentieth-century urban problem, from poverty, pollution, congestion, riots, and crime to suburban sprawl and alienation; a great manufacturing center hardly affected as yet by the new techniques of the industrial revolution; a city with a mass electorate long before 1832 and



with radical organizations decades before the French Revolution; a city undergoing all the most unsettling stresses of the modern megapolis, yet universally admired by contemporary foreign visitors for its wealth, cleanliness, and order. We need both more specialized studies and more re-examinations of received opinions on eighteenth-century London, to make Rudé's book, admirably though it sets forth the present state of our knowledge, rapidly obsolete.

DONALD J. OLSEN  
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FRIDA KNIGHT. *University Rebel: The Life of William Frend (1757-1841)*. London: Victor Gollancz. 1971. Pp. 320. £3.00.

Other than one of five columns in the *DNB*, there was no biography of Frend until Mrs. Knight's. She discovered a trunk full of his correspondence that provides detail for the story of his life. A fellow of Jesus College, Cambridge, and an able mathematician, Frend suffered banishment in 1793 after the vice-chancellor's court and the senate judged that his book, *Peace and Union*, offended against a university statute, meaning against the temper of the times. The account of this proceeding, meant to be the high point of the book, is the most disappointing part of it. Mrs. Knight spoiled her opportunity by relying entirely upon Frend's published version of the case, by her unnecessary partisanship, and by her inadequate scholarship. Frend's tactless conduct as a university rebel obscured the academic freedom issue and made his defeat predictable. Thereafter in London he lived among political radicals, dissenters, and literary people. The discussions of Frend's personal associations at Cambridge and in London are the best parts of the book. Frend maintained these personal connections and his interest in good causes even after 1806 when he entered upon a twenty-year career as actuary for the Rock Life Assurance Company. Mrs. Knight accounts for this career in one badly garbled page. By this time the book has gotten away from her. In middle and old age, Frend had a busy and satisfying life as husband, father, actuary, journalist, reformer, and friend of many of the literati, but all of this appears in sketchy form without depth and substance. A biographer of Frend needs to

know more of the history of Frend's times than Mrs. Knight bothered to learn. Instead of writing a very good book, she only wrote a pleasant one about a man who was actually bigger than the person she presents. Still, whoever undertakes to write a more substantial biography of this interesting man will find it difficult to bring out Frend's personality and character better than Mrs. Knight has done.

CARL B. CONE  
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D. C. M. PLATT. *The Cinderella Service: British Consuls since 1825*. [Hamden, Conn.:] Archon Books. 1971. Pp. x, 272. \$11.50.

Described as "the first comprehensive work" on its subject, this book breaks new ground in a little known field. The British Consular Service became a government service in 1825, but consular services on a private basis had begun much earlier. The book deals with administrative and social aspects, which supply almost the only firm ground for a historical survey. This was a relatively minor government service whose miscellaneous activities, though cumulatively important, left little record. Scattered in five continents and working as individuals isolated from each other, consuls had only the most tenuous relations with London, the seat of power, prestige, and patronage. Their miscellaneous functions, poor reporting, the slow mails, the lack in London of administrative oversight of their work, and the absence of fixed policies robbed the records of threads of continuity. The book makes extensive use of archival material, including consular reports, the evidence, minutes, and reports of investigating committees (which mostly left things as they were), parliamentary debates, and consular memoirs.

To have discerned the continuities and interest in such a chaos of particulars and to have digested it into a short, tightly packed book is a real achievement. Footnote references to sources are meticulous, and there is a good bibliography. The author's eye for essentials, the economy and lucidity of the writing, and the wide and careful research impress the reader. The unifying theme of the "Cinderella Service" adds interest to the book.

Two long chapters deal with the General

Service in the nineteenth and twentieth centuries. Two more, each of special interest, deal with the Levant and the Far Eastern services—both in international frontier areas of primitive administration and weak sovereignty, where Western powers sought to regulate contact by capitulations and extraterritoriality. Each chapter contains succinct subsections dealing comparatively with such matters as functions, recruitment, training, service conditions, salaries, control, and inspection. Political and judicial functions and extraterritoriality are emphasized in the two regional chapters.

The account of the amalgamation in 1943 of the Foreign Office, Diplomatic, Commercial Diplomatic, and Consular Services into a single Foreign Service brings the "Cinderella" theme to the forefront. It is the major emphasis of the preface and introduction and recurs in each chapter. In the epilogue the author avowedly becomes "partisan," exchanging "the role of historian for that of social commentator and critic." The consul was the victim of "social injustice," kept down by "the social distinctions and snobberies" of the Foreign Office and the highly privileged diplomats. Amalgamation instead of securing "equality" left the consul weaker. He still suffered from the "contempt" in which "for a century and a half the job itself was held." There was a time when "no self-respecting Foreign Office clerk or diplomat would have been seen dead with a consul as colleague." A historian bent on defending the underdog may risk going out too far on thin ice.

H. DUNCAN HALL  
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J. F. C. HARRISON. *The Early Victorians, 1832-1851*. New York: Praeger Publishers. 1971. Pp. xix, 195. \$10.00.

For historians raised on John and Barbara Hammond's compassionate picture of England during the Industrial Revolution, J. F. C. Harrison's *The Early Victorians* will evoke pleasure and nostalgia. Harrison once again describes the social experience of the Industrial Revolution with a Hammondesque vividness and sympathy. The old landmarks are still prominent: the Industrial Revolution, population growth, urbanization, the factory system,

King Cotton, and the dramatic cycles of prosperity and depression. The emphasis, of course, is on poverty and squalor. Harrison will not allow, any more than will the Hammonds, posterity to forget the starving frame knitters, naked coal hewers, inebriated navvies, and harassed menial servants; nor will he allow posterity to overlook slums whose intense overcrowding, bad water, and lack of sewerage meant high death rates, sexual promiscuity, drunkenness, and misery. In dramatic contrast to this squalor is the life of the upper classes.

Harrison's picture of the aristocracy emphasizes the elegance of their country homes, their local power, their foxhunting, and their use of spring traps and transportation to rid themselves of poachers. His picture of the middle class underscores their nouveau riche passion for fine homes, fashionable clothing, and many servants. Both classes expected deference from the poor. In press and pulpit they preached doctrines of self help and religious seriousness and advocated the gaol and the New Poor Law's grim workhouses for the really erring and lazy.

Harrison, in drawing again this picture of exploitation, has done a service for historians. However familiar this story of harshness and inequality, it is one that deserves retelling. But Harrison has done more than redraw a picture. He has also added refinements and discriminations. His treatment, for example, of class structures is particularly discerning. He goes beyond the simple upper-, middle-, and lower-class schema to delineate the many and varied divisions that made up the social complexity of Victorian England, divisions in which a well-paid iron foundry worker lived better than a poor property-owning green grocer. It is Harrison's particular descriptions of these various classes and how they fitted into the complex network of Victorian society that, along with insights on social movements and social attitudes, give this work some originality.

It is thus all the more regrettable that he has not purged himself entirely of a Hammondesque disposition to exaggerate suffering and oversimplify oppression. On page 81 he says of the New Poor Law of 1834, "outdoor relief was to be abolished and all recipients made to enter the workhouse." To remind Mr. Harrison that the New Poor Law said nothing at all

of a workhouse test might be pedantic, but it is not pedantic to remind him that the Poor Law Commissioners (whose orders, not the law, defined the workhouse test) never imposed it on every applicant. They always included in their orders classifications for those paupers who were entitled to outdoor relief. Never did they make "all recipients" enter the workhouse. This error is not an isolated one. Harrison's work is full of exaggerations, ranging from the mistaken notion that the early Victorians were "firmly in the grip of Malthusianism" (p. 6) to pictures of sexual promiscuity in factories, seven-shilling weekly wages in Wiltshire, and naked coal hewers. There were, to be sure, Malthusians, sex in factories, seven-shilling wages, and naked coal hewers in early Victorian England, but a careful reading of Kenneth Smith's *The Malthusian Controversy* (1951) or of the government reports on factories, agriculture, and mining will show that they were the exceptions. Harrison's admirable sympathy for suffering has led him to mar his largely accurate and often perceptive study with oversimplifications. This is unfortunate since the socialist tradition in English historiography, which has done so much to illuminate the reality of misery and oppression, has always been attacked for its biases and distortions. To free that tradition from those charges, historians as important as Harrison and as famous as E. J. Hobsbawm, general editor of this series and a reader of the book in manuscript, must be much more accurate and balanced.

DAVID ROBERTS  
Dartmouth College

C. H. S. FIFOOT. *Frederic William Maitland: A Life*. (Studies in Legal History, published in association with the American Society for Legal History.) Cambridge, Mass.: Harvard University Press. 1971. Pp. x, 313. \$10.00.

Mr. Fifoot has put the whole historical world in his debt by publishing an edition of Maitland's letters in 1965 and now by writing the best life of Maitland we could ever have. Miss Ermengard Maitland asked him to write it and made available to him all the family papers and a rich yield of family memories. Maitland was born in London in 1850; two sisters were born in 1849 and 1851. The family had al-

ready, by marriage, acquired land in Gloucestershire. Maitland's grandfather Samuel Roffey Maitland came from a Scottish family with strong Nonconformist leanings, so that, although he went to Cambridge, he did not subscribe to the Thirty-nine Articles and could not take orders, but by the Easter term of 1816 he became a barrister. Scholarship and music were both more attractive to him than the law. He abandoned both the law and the Church but gave himself to the critical study of Evangelical writers. Maitland himself said "that we still have to do for legal history what S. R. M. did for Ecclesiastical history. His *Facts and Documents* is the book I admire most. . . . It teaches us that a statement does not become true because it is constantly repeated. A chain of testimony is never stronger than its first link."

Samuel Maitland had one son, John Gorham, born 1817, who had a distinguished career at Cambridge, third classic of his year, seventh wrangler, second Chancellor's medalist, and a member of the Apostles. He was called to the bar but, like his father, did not practice. He wrote a few pamphlets on income tax and became secretary of the Civil Service Commission. His wife was a daughter of John Daniell; she died while her youngest child was still a baby. Gorham died in 1863, but he and Samuel (who died three years later) had both added to the family money so that after providing for the two girls in the usual fashion of the day, it sufficed to give F. W. Maitland both a good education and "the sense of stability and independence which in itself justifies private income. But he inherited from his paternal grandfather more than land and money: a keen and original vision, intellectual powers, both wide and deep, moral courage and a resolve never to do less than his best. His was a rich inheritance."

But Maitland was not a prominent Etonian either in play or work; he hated the classics and felt cricket and football rather slow, but loved the river and walking. The same tastes dominated his Cambridge years. He was elected to the Chitchat club and in due course to the Apostles and to the Sunday Tramps, where he met Vinogradoff.

During the ten years that followed his success in the tripos in 1872 Maitland came to re-

alize that his future lay with the history, not the practice of the law, but had not Henry Sidgwick provided £300 a year for four years for a readership in law, nothing might have been done to help him. That Sidgwick meant Maitland to benefit seems certain although Mr. Fifoot does not actually say so. The 1880s were the years of decision for Maitland's public and his private life. At Leslie Stephen's house he met Florence Fisher, a sister of H. A. L. Fisher. She and her sisters could not have much spent on their educations with seven brothers to be launched on the world. She had had a country upbringing, which was to stand her in good stead in the Grand Canary. She was, moreover, a lover of music, and "a splendid player on the violin, viola and piano." When they were married Maitland was thirty-six years old and Florence was twenty-two. After a Devon holiday they set up house in Cambridge for the autumn term. In November the Selden Society came into being, largely through the energy of Maitland, but it was unfortunate that P. E. Dove, a barrister who was struggling unsuccessfully to collect work, was left too much power over the funds of the society. In 1894 he found that he owed the society £1,000 and that the society owed a large bill to the printers. Before the society had been in existence for ten years Dove gave up hope of pulling things together and committed suicide.

To a historian Mr. Fifoot has settled conclusively what has always been a disturbing problem: how far ought we to accept Sir Frederick Pollock as the effective collaborator with Maitland in writing *The History of English Law*? That question is now settled once and for all. This great book is Maitland's as we always felt that it was. Pollock's hand need only be traced in the section on Anglo-Saxon law where even a new edition could not entirely eradicate the unsuccessful efforts of Pollock.

The happy family life of the Maitlands was continually overshadowed by the threat of illness. After much discussion they determined on the Grand Canary as the most convenient place to go. Save for the winter 1904-05 they went there every year. Despite constant illness, Maitland never refused any challenge whether it came from the Deputy Keeper of the Public Records and the Year Books or the family of Leslie Stephen and the duty to write a book to

preserve his memory. When he died in 1906, Maitland had done far more than most men have ever done in a longer life.

†DORIS MARY STENTON

J. K. CHAPMAN, editor. *A Political Correspondence of the Gladstone Era: The Letters of Lady Sophia Palmer and Sir Arthur Gordon, 1884-1889*. (Transactions of the American Philosophical Society, New Series, Volume 61, Part 2.) Philadelphia: the Society. 1971. Pp. 52. \$2.00.

History's losers have a nostalgic fascination about them. Often good people in terms of their own time, they are overtaken by events and changes that they have little capacity to control or even understand. In public they may put up a forthright defense, but in private they reveal their apprehensions and dismay.

Lady Sophia Palmer and Sir Arthur Gordon were such people. As the editor of their letters says, they were "conservative Liberals, not themselves leading politicians." Products of the mid-Victorian political elite, they viewed with increasing alarm the democratization and radicalization of English politics in the 1880s and of their leader, Gladstone.

They are interesting not so much because they "counted" but because they knew or were related to a great many people who did. Lady Sophia was the daughter of Gladstone's Lord Chancellor, Lord Selborne, and by family and acquaintance she was connected to leading political figures in both parties. Sir Arthur was the youngest son of the fourth earl of Aberdeen. After a political apprenticeship as his father's private secretary, he became a brilliant colonial governor over a period of thirty years, always imperiously preferring the Crown colonies to the self-governing ones.

The correspondence of Lady Sophia and Sir Arthur, scarcely any of which has previously been published, extended from 1880 to 1912. Professor Chapman has made a selection from their political letters from 1884 to 1889, along with two letters of Lady Sophia on the general election of 1880 and the death of Gladstone. Her letters, because she was in England, have more detail on people and their private views, while his, mostly written from the distance of colonial assignments, are mainly reflective discussions of political issues. The letters are generously annotated, but it would help to know

which ones Gordon wrote when he was out of England.

Not much is added to our knowledge of political events here, although a good deal is confirmed. More interesting are the central themes of fear of the future and betrayal. Whether the issue be socialism, the franchise, Ireland, sanctity of property, or the Church, both correspondents reveal a sense of doom. They had little recognition of the economic and political stresses reshaping the country, and Gordon could only despair at "a large, inert mass ready, though not willing, to be revolutionized" by "the few who really desire revolution." The villains were Chamberlain, Churchill, and most of all Gladstone "whose dangerous demagogic gifts did untold harm latterly." But with a forgiveness that ironically revealed her Victorian anachronism, Lady Sophia could say of Gladstone at his death, "Yet after all the *real* power of the man lay in his faith in God."

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University of Oklahoma

G. R. SEARLE. *The Quest for National Efficiency: A Study in British Politics and Political Thought, 1899-1914*. Berkeley and Los Angeles: University of California Press. 1971. Pp. x, 286. \$8.50.

G. R. Searle's study of the political ideal of "national efficiency" attempts to transcend the political historian's conventional concern with party, institutions, and policy. But Searle is most persuasive as a traditional political historian successfully tracing the complicated course of Lord Rosebery's bid for national leadership from 1900 to 1902, the Education Act of 1902, the Webbs' Poor Law Minority Report, and Lloyd George's abortive efforts toward a national coalition government in 1910. To identify a "national efficiency group" Searle argues that a reaction to Britain's decline as a great power in the last quarter of the nineteenth century, exacerbated by disillusionment with Liberal political forms, led to a "corporate identity" among such disparate political figures as the Webbs, H. G. Wells, G. B. Shaw, R. B. Haldane, A. J. Balfour, Lord Rosebery, Alfred Milner, Robert Morant, L. S. Amery, and Lord Esher. To these people Searle attributes a common ideology of imperialism, Broad Church traditions of social thought idealizing the Ger-

man state, reliance upon experts in government, and state-aided science and technology.

Searle's criteria for membership in the "national efficiency movement," evidently derived from the Webbs' analysis of inefficiency and their bureaucratic and suprapolitical remedies, are applied only to those eager to promote national strength. This treatment of ideas qua programs introduces substantive and methodological problems. An individual's use of politically popular rhetoric in the decade following the Boer War is not sufficient evidence that he belonged to an "efficiency group," especially since the same priorities and goals were advocated by liberals whom Searle puts in opposition to this group. C. F. G. Masterman and Herbert Samuel within government and Alfred Marshall, J. A. Hobson, and L. T. Hobhouse without had been serious and influential critics of wasted human resources before the Boer War made national reform a popular issue. The political activities of men like Balfour and Haldane, furthermore, cannot be discussed apart from the intellectual and spiritual assumptions that governed everything they did. Searle's exploration of politics and political thought exclusively in terms of the machinery of government ignores the development of political thought and conduct as part of a larger revolution in social, economic, and political theory and practice occurring from the 1880s to 1914 in the closely related communities of scientists, men of letters, members of parliament, dons, bankers, and businessmen.

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San Fernando Valley  
State College

P. F. CLARKE. *Lancashire and the New Liberalism*. New York: Cambridge University Press. 1971. Pp. ix, 472. \$18.50.

DAVID AYERST. *The Manchester Guardian: Biography of a Newspaper*. Ithaca: Cornell University Press. 1971. Pp. 702. \$15.00.

The center of Clarke's argument is (1) Lancashire was one of the critical areas of political geography (Greater London was another) where the post-Gladstonian Liberal party had to make gains in order to win a general election; (2) the party succeeded here, not only in 1906 but in the 1910 elections; and (3) its suc-



cess in working-class constituencies here showed how strong and healthy the party was on the eve of the 1914 war. So much for all those from George Dangerfield to Roy Douglas who have argued the inevitability of Liberal decline by 1914.

The case for Liberal strength is not made simply in terms of the number of seats held by Liberals through the elections of 1910 (indeed Clarke shows that by December 1910 the Conservatives had doubled the number of seats they had in 1906 in the northwest). It is the fall in the Conservative poll that indicates their relative weakness; another sign is the marked Liberal success in the "more highly enfranchised" seats (that is, those with over sixty to sixty-five per cent of the adult males on the register).

All the same, the author concedes that what would have happened in a 1915 election, without a war, is "an open question." By restricting himself to Lancashire and the northwest he skips the fact that seven Liberal seats lost to Conservatives from 1910 to 1914 were the result of three-cornered contests. And surely it would have surprised the Webbs to hear that the term "progressive" has been "consigned to . . . American history." This is a serious, if very expensive book, with an excellent bibliography.

The chronicler of Manchester's *Guardian* proceeds with unhurried, affectionate detail from the aftermath of Peterloo to the Suez crisis of 1956. He relies upon the files and archives of the *Guardian*, including letters by its editors, and especially the letters of C. P. Scott. Over three-quarters of the book is devoted to the age of Scott (editor 1871-1932, or effectively 1871-1926) and after. The golden age began after the Boer War but before Scott's purchase of the paper in 1907.

This is a labor of love without any easily incapsulated thesis. There are fascinating anecdotes: Admiral Lord Fisher wanted Redmond made prime minister in 1916; before the Balfour Declaration the leader-writer Herbert Sidebotham saw the advantage of a Jewish Palestine (as a British dominion) safeguarding the Suez Canal; on Captain Walter Lippmann's advice, Scott tried to hire first Gilbert Murray and then his son-in-law Arnold Toynbee as an American correspondent in 1919.

Laurence Scott, grandson of C. P., found out in 1950 that *Guardian* readers all over tended to be well-educated people who cared much more for international politics than for local cricket. In 1959 the masthead read simply *The Guardian*; in 1960 the first London printing was run off. This is a good book about an admirable journal. Unhappily the footnotes are buried in the back, under chapter numbers.

BARRY MCGILL

Oberlin College

ARTHUR J. MARDER. *From the Dreadnought to Scapa Flow: The Royal Navy in the Fisher Era, 1904-1919*. Volume 5, *Victory and Aftermath (January 1918-June 1919)*. New York: Oxford University Press. 1970. Pp. xxiii, 416, 7 charts. \$12.00.

In this concluding volume of his magisterial series on the Royal Navy in the Fisher era, Professor Marder has to handle three different themes: the triumphant conclusion of the war at sea; the readjustment of the navy from war to peace; and a summary, not only of the five volumes of this series, but of a scholarly lifetime devoted to the senior service. All are handled with the precision, clarity, and careful analysis that have marked the preceding volumes.

The most dramatic episode in the first part of the book is the surrender of the High Seas Fleet to Sir David Beatty's Grand Fleet on November 21, 1918, but the chapter that will be read most attentively is doubtless that which describes the final defeat of the submarine campaign. Marder's analysis and the statistical tables accompanying it demonstrate clearly the point made in volume 4: convoy was the answer, and in the circumstances of 1917-18, the only answer to the U-boat. Other expedients, such as the vast northern mine barrage and the spectacular Zeebrugge raid, had only the most fleeting and marginal effect. Yet the belief that convoy was defensive and therefore not consonant with sound principles of maritime strategy died very hard. The battle between the advocates of convoy and hunting groups had to be fought out again in 1939-41. The entire story of the convoy system and its vicissitudes is an instructive example of the weak staff work at the Admiralty as well as of the deficient intellectual grasp of strategy and



tactics by many senior naval officers—two characteristics that Marder highlights in his concluding chapter, “Reflections on an Era,” as some of the worst failings of the Royal Navy during the First World War. It also illustrates another of Marder’s conclusions: the unfortunate effect on the navy of the belief that the Nelson tradition required constant offensive action. In terms of the U-boat menace this translated into futile offensive sweeps a policy that, if persisted in, would have led to disaster. It was unfortunate that Sir James Saumarez was not given a small tithe of the attention that went to Nelson.

During the period between the armistice and the signing of the Versailles Treaty, the naval settlement with Germany occupied a great deal of Admiralty attention, as did demobilization, reorganization, and the naval aspects of the allied intervention in Russia. The two events of greatest significance for the future, however, were the “sea battle of Paris”—the clash between the British desire for primacy at sea and the American demand for a “navy second to none”—and the Admiralty’s recognition that Japan was the most likely naval threat in the future. The story is taken up from this point on (with somewhat less clarity) in Captain S. W. Roskill’s *Naval Policy Between the Wars: The Period of Anglo-American Antagonism, 1919–1929* (1969), but by the time the High Seas Fleet scuttled itself at Scapa Flow on June 21, 1919, enough had happened to make it clear that that event was the end of an era rather than the prologue to a new age of British supremacy at sea.

Marder’s conclusions about the Royal Navy in the Fisher era lead the reader to reflect on Marder’s achievement. It is not very likely to be bettered or supplanted for a long time to come. The recent publication of the third volume of the official Churchill biography by Martin Gilbert (*Winston S. Churchill: the Challenge of War, 1914–1916* [1971]), with copious extracts from the Churchill papers, does not substantially modify Marder’s evaluation of Churchill’s tenure of the Admiralty in 1914–15. On the *Goeben* episode, for instance, Marder’s discussion is considerably fuller than Gilbert’s. In fairness, Mr. Gilbert’s declared intention is to rely exclusively on contemporary documents. An eight-hundred-page book largely devoted to

Admiralty affairs that does not once mention Marder’s work, however, will strike many readers as remarkable. If this partial opening of the most important remaining private archive dealing with naval affairs during the First World War does not necessitate any serious modification of Marder’s work, it seems safe to assume that nothing else will. Not everyone will share his great enthusiasm for “Jacky” Fisher or his more restrained enthusiasm for Jellicoe. Additions and corrections to the record will continue to be made. The work as a whole, however, is indispensable to our understanding of the last era of British supremacy at sea and a splendid monument to the ships and men who sustained it.

RAYMOND CALLAHAN  
University of Delaware

MAURICE COWLING. *The Impact of Labour, 1920–1924: The Beginning of Modern British Politics*. (Cambridge Studies in the History and Theory of Politics.) New York: Cambridge University Press. 1971. Pp. ix, 570. \$17.50.

This book could have been the most notable contribution to our understanding of modern British politics. With all its faults it must still be considered one of the truly important studies in the field. The first to discuss the response to Labour from 1920 to 1924 and to show the extent to which Labour became “the major problem” for Liberals and Conservatives, it is also the first to explain that, with respect to Labour, the struggle within the Conservative party concerned tactics rather than policy and that the Conservatives who defeated the Coalition in 1922 were just as opposed to socialism as those who wished to remain allied with the Lloyd George Liberals. So, it seems, were the Asquithian Liberals who wanted to organize a center party in order to provide an alternative other than Labour to Lloyd George. Indeed, once the Coalition fell all Liberals—particularly Lloyd George—wanted to establish themselves as the alternative to the very same threat.

Why they failed and the Conservatives emerged as the party of constructive resistance forms a second, almost parallel theme. One might say themes, except that they are linked through the person of Stanley Baldwin whose political genius and studied moderation Cowl-

ing freely admits. In doing so he tacitly questions some of the sentimental myths surrounding the Conservative leader. More explicitly, he anticipates Roy Douglas, *The History of the Liberal Party* (1971), in suggesting that the Liberals' decline, far from being inevitable, was actually the result of astute tactics on the part of Baldwin as well as internal divisions and a series of miscalculations culminating in Asquith's decision to put Labour into office in 1924.

The story of this decision, like the rest of the book, is drawn from many private papers. Interspersed are amusing, if sometimes oblique, descriptions of the politicians involved. Yet the detail can be overwhelming and difficult to follow because *The Impact of Labour, 1920-1924* is poorly constructed and because its parallel themes—Labour's threat and the success of the Conservatives in meeting it—unfold in confused, uneasy tandem. The second yields long lists of feuding factions whose relevance to the problem of how best to deal with Labour is not always made clear. The basis of the problem is itself unclear and, for this reason, so are the grounds for the tactics adopted by the victorious moderates.

Take Baldwin's conscious effort to destroy the Liberals in 1924. One might have thought that in rejecting coalition as an answer to Labour, he would have looked to a revived Liberal party. His decision to adopt a protectionist platform in 1923 helped to revive it. But Cowling denies that the effect was intended without saying when Baldwin determined on Liberal collapse or when he came to accept either the eventuality or the possibility of Labour rule. At times he suggests that the leadership of Ramsay MacDonald after 1922 made the difference; at other times he suggests that Black Friday and the collapse of direct action in 1921 counted most. Either way there are problems. Leaders of the Labour party had never endorsed direct action and, contrary to what Cowling believes, their acceptance of parliamentarism did not begin in 1921. Whatever changes in policy occurred after that date cannot be ascribed to new methods of achieving them: if the changes were as great as Cowling implies, then it is hard to see why everyone was so fearful in 1922, and if the significant change came after 1922, then Cowling must prove that

MacDonald differed substantially from previous leaders of the party.

This he cannot do. His emphasis on the threat of Labour may be a useful corrective to Robert Skidelsky, *Politicians and the Slump* (1967), and others who minimize the differences among parties. But in the end he does not explain how great those differences were, whether Labour's threat was as real as it was perceived to have been, or why the conclusions drawn from the same perception differed so widely. Such omissions are serious in a book about the impact of Labour. Even more serious, perhaps, is his failure to discuss the constitutional implications of all the proposals for some sort of coalition or center party to keep Labour permanently out of office.

BARBARA MALAMENT  
Yale University

NOREEN BRANSON and MARGOT HEINEMANN. *Britain in the 1930's*. New York: Praeger Publishers. 1971. Pp. x, 358. \$11.00.

The excerpts are the best part—Sir John Boyd Orr speaking the truth on poverty and malnutrition, three angry Conservative M.P.'s reporting the brutalities of a Mosley rally, and a Rhondda Valley miner recalling how he and his mates stole coal and timber from the mines. When the authors let the contemporaries speak *Britain in the 1930's* comes alive. Nearly all else is disappointing.

The social and industrial topics skimmed in this discursive book have absorbed and continue to absorb the energies of dozens of specialists; yet for recent findings one searches in vain. For detailed accounts, meaty and authoritative, one must still turn to monographs long known. For synthesis one does far better with Charles Mowat's *Britain Between The Wars*, published in 1955.

Best done, I thought, are chapters on "The Unemployed and the Means Test," and on malnutrition ("Eating and Not Eating"). But the chapter on "The Radical Trend in Culture" remains a catalog of names. The subject of "Industrial Graveyards" presented an opportunity to show, in vivid human terms, what chronic unemployment meant; but the opportunity went by default, buried among generalities and statistics. Surprisingly weak is the

chapter on "Class Structure and Class Outlook," which one would hope to be particularly strong in a series designed "to bring together . . . what we know . . . about the structure and changes in British society" (p. viii). The chapter on "Homes, Landlords, and Building Societies" does not begin to suggest either the complexities of housing or the ferment of ideas through the decade. Two excellent chapters discuss industrial unions and their organizational efforts. The authors succeed in highlighting several themes: slowness of mass organization, lethargy of union leadership, the impact of the unemployed marches, the continued decline in workers' share of output despite rising productivity, persistence of stratified inequalities in income and spending power, and official apologetics, now evasive and now callous, for inaction.

Professedly "social history" concerned "primarily with the everyday lives and attitudes of the British people" and the "ordinary person," this book veers to the political often. The process is never haphazard. Attention focuses on the more striking efforts of Communists at the grass roots or on the more egregious (and, by implication, typical) profascist comments from the upper class. One result is to leave virtually unexplained the government's eventual liberalizations or modifications of policies originally callous or miserly. The book's credibility gains nothing from this partisanship.

PAUL B. JOHNSON  
Roosevelt University

FRANKLIN REID GANNON. *The British Press and Germany, 1936-1939*. New York: Oxford University Press. 1971. Pp. x, 314. \$10.50.

Gannon's first chapter may be described as an essay in minor revisionism devoted to a subject, appeasement, that has largely escaped such treatment. This substantial study is the product of strenuous effort. The revisionism is minor in scale because the author is not primarily concerned to impugn the present by subverting its vision of the past. Instead the author argues that appeasement was the result of British reaction to the Nazi regime as well as the result of a complex of attitudes dividing Britain's Left and Right—and splitting each of them. Some of these divisive problems persist

"to bedevil both contemporary politics and the possibility of a detached consideration of the inter-war period" (p. 31).

The revisionism largely consists of denying the foresight of Leftist liberals and the guilty propensity of the Right for Hitler as a mighty fortress against Bolshevism. There is nothing wholly new in this except for the sympathetic presentation of the Rightist position. Gannon's background setting presents the press competing for circulation. They were therefore unwilling to worry readers severely by drawing conclusions that might call for a costly response. An understandable but quite unheroic discretion was compounded by the fear of jeopardizing Britain's frail economic recovery. Left and Right were largely in agreement—"never again"—about World War I: the former favored the League of Nations, disarmament, the expression of outrage against Hitler, and, in smaller numbers, a national and international popular front; the Right favored rearmament, the British Empire, and the prospect of a settlement with Germany. At various times parts of both groups distrusted France and larger numbers thought the Versailles Treaty illegitimate and regarded Czechoslovakia as its unloved scion.

Gannon's pages convey the journalistic and editorial thoughts and sentiments in which appeasement flourished. His account of a selected number of London papers (as well as the *Manchester Guardian*) is interesting but somewhat random. Purists of method will not accept his rule-of-thumb justification of his selection. Those with a taste for drawing parallels with our times may find reading this book a haunting experience. But for all its interest the book is disappointing in the coherence of its design, in the inadequate development of the theme that appeasement marked a crisis in the conscience of liberalism, and in its impoverished concluding section.

M. A. FITZSIMONS  
University of Notre Dame

K. THEODORE HOPPEN. *The Common Scientist in the Seventeenth Century: A Study of the Dublin Philosophical Society, 1683-1708*. Charlottesville: University Press of Virginia. 1970. Pp. xii, 297. \$7.50.

Ireland produced few important seventeenth-

century scientists; one of the more interesting ones, however, was William Molyneux (1655–98) of Dublin, a friend and correspondent of John Locke and Edmond Halley. It was Molyneux who worked to galvanize the meager scientific resources of Ireland and to establish a working scientific society for Dublin.

It is a widely held belief, and basically a sound one, that important elements of the scientific revolution of the seventeenth century are epitomized in the numerous societies for the pursuit of science that sprouted during the course of it. The organization that has justifiably commanded and continues to garner the lion's share of attention has been the Royal Society of London. In his study K. T. Hoppen chose not to re-evaluate the basic premises of the literature but rather to examine in depth an offshoot of the London group. The Dublin Philosophical Society, founded in late 1683 and early 1684, offers a striking opportunity for the examination of the concerns, goals, and methods of late seventeenth-century science. We possess in the records of the Irish group an accessible and fully documented case of a scientific society consciously organized upon what was conceived to be the principles of the Royal Society of London. We have here, as it were, a model of the Royal Society, stripped of its complex and obscured origins, its historical accidents, and its warring factions. In short, the Dublin Society provides a clear reproduction of the public face of later seventeenth-century science.

Hoppen has diligently collected the minutes, letters, and other papers pertinent to the Society, along with an impressive bibliography of secondary works; the sum provides an image of exhaustive and painstaking research leading to what will doubtless stand as the definitive description of the structure and function of the Dublin Philosophical Society.

Yet from the point of view of the historian of science, there remains something more to be done. The heart and soul of the Dublin Society was Molyneux, who, although best known for his single political tract, devoted an overwhelming part of his efforts to astronomy, dynamics, and optics. Molyneux left enough scientific correspondence (with the astronomer Flamsteed and with Halley, for example) and other manuscripts to keep a small team of scholars busy with new and exciting material.

Hoppen makes no concerted attempt in this book, however, to dig deeply into the character and value of Molyneux's achievement. Molyneux's very interesting translation of much of Galileo's *Dialogues concerning Two New Sciences* is passed off (pp. 129–30) as the *Dialogues concerning the Two Chief World Systems*.

But if Hoppen has not plumbed the depths of the science carried on in seventeenth-century Ireland, he has produced a polished, detailed, and valuable account of the organization and development of a scientific society that will surely provide part of the groundwork for that much-needed reassessment of the character and role of cooperative scientific efforts in the scientific revolution.

ROBERT H. KARGON

*Johns Hopkins University*

GEOFFROY ATKINSON and ABRAHAM C. KELLER. *Prelude to the Enlightenment: French Literature, 1690–1740*. Seattle: University of Washington Press. 1970. Pp. 221. \$7.95.

This book is the last of three volumes by the late Geoffroy Atkinson on French literature in the period from 1690 to 1740. It is presented with interpretive commentary as a collection of quotations ("the quotations, and not our comments, are the meat of this book") that Abraham C. Keller has translated and selected from Atkinson's notes.

The authors stress the diversity of literary themes and eschew stuffing the Prévosts and Marivaux into categories but emphasize those themes that show a break from the classical age and anticipate Romanticism. Thus, in part 1 ("The Emotional Revolution") the excerpts reveal a penchant for wallowing in emotion that the generation of Racine would have found comical or ridiculous but that to later writers such as Bernardin de Saint-Pierre would have seemed natural. Aspects of the "emotional revolution" such as sentimentality, personal confession, and primitivism indicate how "lesser writers first cultivated the genre and prepared the public taste for the master-work of Jean-Jacques [Rousseau]." The strongest evidence of a shift in literary values was in the libertine view of love that presaged the romantic attitude of the later eighteenth century.

Themes commonly associated with the Enlightenment included toleration of intellectual

and religious diversity, freedom from dogmatism, skepticism of revealed religion, cosmopolitanism, sympathy for the poor, exoticism, belief in progress and perfectibility, social and racial equality, and the utility of science. In part 2 ("The Broadening World of 1700") Atkinson and Keller find these themes in such obvious writers as Bayle, Montesquieu, the lesser-known Jean Buvat, and the anonymous authors of the *Journal des Sçavans*. While many motifs of the Enlightenment are audible in its prelude, the authors discern more emotionalism than rationalism, more that anticipates Romanticism than the Age of Reason.

Central to the authors' purpose is the attempt to pin the literary innovations of 1690-1740 to the taste of a growing bourgeois audience. Achieving unusual acceptance by all social classes, the intellectuals wrote "quite consciously, for the enjoyment of their fellow commoners, often with open or tacit disregard of the approved values of traditional aristocratic society." Their novels, which preached a plain ethic of benevolence and personal happiness, helped to make the daily life of commoners an object of sympathy.

The principal strength of this book is the careful selection and presentation of a popular literature of sentiment, exotic adventure, revelations of the bedroom, and even social protest. The authors discover that the literature of science and reason found a smaller audience than did the literature of *sensibilité*. But herein, too, lies the book's weakness. The authors fail to analyze the audience for these books beyond the amorphous middle class. Who bought which books and in what quantity? In his preface Professor Keller has forsworn a statistical analysis of book sales. But he makes the matter of the audience so crucial to his discussion of the literature, and he so tantalizes the reader in his conclusion with the information that Atkinson possessed "considerable data about writers, publishers, and public acclaim or failure" proving that "audiences were much larger and more bourgeois in their make-up than in the heyday of the reign of Louis XIV," that one is left begging for a social analysis of this public and its taste. One is tempted to propose that Professor Keller write yet a fourth volume.

ROBERT ISHERWOOD  
Vanderbilt University

CHARLES COULSTON GILLISPIE. *Lazare Carnot, Savant*. A monograph treating Carnot's scientific work, with facsimile reproduction of his unpublished writings on mechanics and on the calculus, and an essay concerning the latter by A. P. YOUSCHKEVITCH. Princeton: Princeton University Press. 1971. Pp. xi, 359. \$17.50.

This handsome volume offers a fresh perspective on Lazare Carnot, the "organizer of victory" in the campaigns of 1793 and 1794 and the only member of the Comité du salut publique to continue in power in the Directory. From the archives of the scientific academies of Paris and Berlin, Dr. Gillispie and Professor A. P. Youschkevitch have recovered forgotten papers that exhibit Carnot in his youth as a military engineer who wrote on mechanics and the calculus in the effort to win recognition in academic competition. His career in politics and government is better known than his real significance as a scientist whose work led to the creation of the laws of thermodynamics by his son Sadi. From the present point of view, his part in public affairs and military administration appears as an interlude; his role in the Terror and his directorate lasted about five years, 1792-97, and his service to Napoleon consisted of the defense of Antwerp in 1812-13 and the ministry of the Interior in 1815. On returning from exile to France in 1800 he resumed public scientific activity, serving on various commissions of the Institut de France, première classe, and publishing in the fields of geometry and mechanics. After Waterloo he went into exile again and died at Magdeburg in 1823.

In 1778 the Académie des Sciences of Paris offered a prize for the most precise statement of "la théorie des machines simples en ayant égard au frottement et à la roideur des cordages," with the proviso that the laws of friction and the effect of the rigidity of ropes be determined by new experiments applicable to naval machinery such as pulleys, capstans, and inclined planes; as none of the essays were satisfactory, a similar topic was announced for 1780. Carnot entered both competitions, and the theoretical section of each paper now published emphasizes the mathematics involved in raising weights, "the purpose of the greater number of all machines actually used." The context of the competition was clearly the three-decked man-of-war, with its numerous



heavy guns and elaborate rigging, requiring powerful hoisting gear, whether in action at sea or under construction or repair in arsenals. Summarizing these essays, Dr. Gillispie writes that Carnot generalized from the principles of action of certain machines to the principles of machines collectively, thereby "making physics out of the industrial reality of the age."

The third essay, presented in full, was entered for a competition established in 1785 by the Académie royale des sciences of Berlin calling for a theory of what is called *infini* in mathematics. Professor Youschkevitch's commentary relates this "Dissertation" to Carnot's later mathematical works as well as to earlier studies of calculus and points out that "a number of the remarkable ideas that Carnot failed to include in the final [published] version of his work have continued to be unknown." Taken in chronological order the three papers show a development in clarity, in the enunciation of principles, and in the definition of quantities; in short, a power of exposition and penetration that was not recognized by the examining committees.

Dr. Gillispie's work illustrates the maturing of the discipline of the history of science from an inclusive gathering of information concerning science and scientists to concentration on specific theoretical advances in the light of immediate circumstances. These three previously unknown documents reveal the widening generalizations that may be expressed, as Carnot himself puts it, in progressively simpler formulations, a kind of language that advances understanding even as it limits such insight to a narrower professional public. This book permits the reconstruction of one man's contribution to the science of mechanics and applied mathematics and thus adds notably to the intellectual history of these critical years. The effect of the whole book is enhanced by the photographic reproduction of crucial texts just as Carnot wrote them, without transcription, translation, or editing. There can be little doubt of what he meant, so clear is his language and his handwriting.

HARCOURT BROWN  
Brown University

GONZALO REDONDO. *Las empresas políticas de José Ortega y Gasset: "El Sol", "Crisol", "Luz"*

(1917-1934). In two volumes. (Colección Rialp, de cuestiones fundamentales, Number 15.) Madrid: Ediciones Rialp. 1970. Pp. 476; 608.

Ortega y Gasset was the dominant intellectual of Spain in this century; the translations of his works were best sellers in many countries. A professor of metaphysics, a journalist, publisher, parliamentarian, philosopher, and humanist, his influence was far reaching. Redondo deals with Ortega as a journalist whose role was inseparable from that of a politician, in a period of change from an oligarchic, liberal, semiconstitutional monarchy to a military dictatorship and then to a republic. Thus the author contributes not only to Spanish history but to the role of the intellectual in politics.

Ortega did not influence events only through his newspaper but was involved in the founding of two political groups. Unfortunately the book does not tell much about these organizations; it centers on the history of three newspapers (*El Sol*, *Crisol*, and *Luz*) that served Ortega and his friends as platforms and reveals the interaction among the editor and collaborators of a journal of opinion, the financiers who owned it, and the government that used subsidies and tariffs to pressure the owners and through them the editorial policy. The conflict between the independent, liberal intellectual and Azaña—the intellectual turned politician—and the realities of party politics in a divided society are analyzed. Ortega attacked the old politics of restoration Spain, hoping that the socialist would play the role of a labor party. Despite his Spanish nationalism he looked sympathetically upon Catalan regionalism. Criticism of the oligarchic two-party system led *El Sol* to find hope in the intervention of the army and Primo de Rivera, only to be soon in opposition to the dictator. The *dictablanda* that succeeded leads to the *Delenda est Monarchia*, the call for a republic, the loss of *El Sol* under the financiers' pressure, and the creation of a new paper, *Crisol* (soon transformed into *Luz*), four days before the proclamation of the republic. Ortega intervened constructively but critically in the constitution making and policies of the Left bourgeois socialist coalition, but seven months later he wrote "No es así," which expressed his disillusion. After giving up his regular journalistic activity he came out with a most insightful analy-



sis of the failures of the Left republicans but also with a prophetic warning to the Right of the mistakes it would make.

Liberal but elitist, conservative but committed to serious reform, opponent of clericalism but also of demagogic anticlericalism, Ortega was soon disillusioned by party politics, pettiness, and demagoguery. The call for a great national party, the appeal and faith in youth, and the critique of party politicians were elements in Ortega's thought reflected in the nascent fascism whose leaders expressed their admiration for him. With his "Viva la República" he still thought that the new regime remained the only alternative. A mixture of economic and political intrigue deprived him of *Luz*; defeated in his efforts to create a new party and to arouse those Spaniards he believed ready for a different style of politics, he returned to academia. This intensely political man, insightful and a perceptive critic, turned silent when he felt that he could not play a responsible and effective role. Less than three years later the Civil War started.

Anyone interested in the contradictions and tragedy of the sincere intellectual in politics, the relationship between economic and political power and the mind, and the difficulties of creating a civil society and democratic politics in Spain will find this book interesting, although long-winded. He will, however, miss an analysis of the relationship between Ortega the thinker and the journalist-politician, the response of others (except Azaña) to his endeavors, and his relationship to those who joined him in his efforts.

JUAN J. LINZ  
Yale University

EDUARDO BRAZÃO. *Présence du Portugal en Belgique (De Philippe d'Alsace à Léopold I.<sup>er</sup>)*. Companhia de Diamantes de Angola (Diamang), Serviços culturais: Dundo—Lunda—Angola. Publicações culturais, Number 80. Museu do Dundo, Estudos de história (ultramarina e continental). Lisbon: the Companhia. 1970. Pp. iv, 18–198.

To those acquainted with Professor Bailey W. Diffie's stimulating study, *Prelude to Empire—Portugal Overseas before Henry the Navigator* (1960), one of the more absorbing aspects of pre-Columbian Europe's developing interest in

maritime commerce was Portugal's role in establishing trade connections with the Low Countries. This subject, usually given but cursory treatment in existing accounts of the late Middle Ages and Renaissance, is now competently and thoughtfully explored by Senhor Eduardo Brazão, the former Portuguese ambassador to Belgium.

This work, originally published in Portuguese in 1969 and now in a French edition, traces the close and continuous commercial and cultural relationships between the Portuguese and Flemish from Portugal's rise under Afonso Henriques in the twelfth century to the ambassadorship of the famed Portuguese poet, Almeida Garrett, to the newly independent Belgium of the 1830s. Its purpose is to underline the strategic importance of Portugal's geographic position as Europe's Atlantic gateway and to emphasize the essential thrust given by early Portuguese-Flemish commerce to the eventual expansion of maritime discoveries sponsored by Henry the Navigator. Carefully researched, fully documented, and excellently illustrated with both photographs and color portraits, the study describes early Flemish interest in Portugal during the Crusades, the formalizing of ties between the two areas in the marriage of Philip of Alsace with Teresa, the daughter of Afonso Henriques, and the subsequent evolution of maritime trade between the Portuguese ports of Lisbon and Oporto with the Flemish ports of Ghent, Bruges, and Antwerp. Originally the midway point on routes from Italy to the North Sea ports and subsequently by virtue of its own much-desired products, Portugal established itself as a major entrepôt in Northern Europe's early trade network. Both in Bruges and in Antwerp its *feitorias* or trading posts became prominent foreign commerce centers that weathered the wars of the Counter Reformation. Their representatives were likewise appreciated foreign residents who developed friendly relationships between the two nations that have endured seven centuries.

Brazão's study attests admirably to the adage once expressed by Belgium's Leopold II that a nation is never small as long as it borders the sea. It also provides much valuable new information on the curious yet significant contribu-

tion of Portugal to the growth of European maritime commerce.

C. J. KOLINSKI

Florida Atlantic University

VALDEMAR ANDERSEN. *Den jyske hedekolonisation* [The Jutland Heath Colonization]. (Skrifter udgivet af Jysk Selskab for Historie, Number 24.) Aarhus: Universitetsforlaget. 1970. Pp. 246. 37.95 D. kr.

Reclamation of swamp, bog, waste, and forest lands in Europe matched rising populations from the 1750s through the whole of the nineteenth century. Its conceptual origins and progress denote, likewise, economic and social changes. Throughout Europe these efforts brought a mixture of populations; Spain, Prussia, Russia, and others sought the unwanted, the dissenters, and refugees from wars or repression, and these colonization efforts exceed the small example of the Jutland case. Denmark reclaimed lands, resettling both its own and German migrants by reason of economic factors alone, for the members of the Rentekammer (tax office) thought that reclamation of the sandy wastes of a small portion of the west-central Jutland coast would profit the state. Immigrants recruited by J. F. W. Moritz from Pfalz, north of Heidelberg, were promised twenty years of freedom from taxation and military service, "day money" of varying sums, monetary assistance for the trip, and provisions to keep them alive. Many harassed Pfaltzers accepted, but the bleak heath of Jutland proved too forbidding and most colonists went elsewhere—to Schleswig-Holstein, Prussia, or other beckoning states.

The persistent colonists were mostly Danes and not Germans, and heath reclamation continued well into the last quarter of the nineteenth century under both private and public auspices. The area reclaimed in the eighteenth century was hardly proportionate to the costs; the assembled sum of 1,046,667 *rigsdaler* of 1783, while reasonable, averaged an enormous amount per acre. Yet these colonizations inspired a willing movement by Danes where costly effort, delayed rewards, and meager living were balanced only by promises.

The most conspicuous weakness of Andersen's book is that the concluding "European perspective" ought to have been a prelude to

explain and place in context this Danish example. It would also have been prudent to examine more fully cameralist and free-trade economic views of such writers as Erik Pontoppidan or J. H. von Justi to test the claim of mercantilist inspiration for the project, for the similar views of its chief supporters—A. G. Moltke and J. H. E. Bernstorff—cast doubts on Andersen's generalization. Likewise, the amount expended on the colonization would deny its value in mercantilist terms and would point to cameralism as its inspiration. The narrow confines of the work within the eighteenth century exclude the next century's major effort and even overlook the agricultural changes transpiring within Denmark simultaneously with the colonization scheme. In other respects the work is competent; an excellent bibliography, lists of immigrants, and reproductions of original documents enhance its value, even if the index is a sort of afterthought.

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JOSEF SEUBERT. *Untersuchungen zur Geschichte der Reformation in der ehemaligen freien Reichsstadt Dinkelsbühl*. (Historische Studien, Number 420.) Lübeck: Matthiesen Verlag. 1971. Pp. 72. DM 15.

Nothing of great moment marks the history of Dinkelsbühl in the late Middle Ages. An average city of about four thousand with a modest *contado* in the Swabian-Franconian region, comfortably wealthy from textiles and metal wares, and possessing a stable mixed government of patricians and guildsmen, the little imperial city entered the Reformation era with typical ecclesiastical conditions suggesting the need for change: too many clerics, an arrogant monastic provost exercising the *jus patronatus* over municipal parishes, and general dissatisfaction with the Church's spiritual guidance. By 1523 Luther had followers among leading tradesmen and politicians, and in 1525 the council began to legislate for the clergy. Widespread citizen support for rebelling peasants brought a brief backlash in 1525 (as it did in nearby Rothenburg), but under the influence of the Lutheran preacher Bernhard Wurzelmann the new religion was firmly established by government action in the 1530s. From then

until the Schmalkaldic War Dinkelsbühl was officially Lutheran, although the old religion never ceased to be practiced. In the reaction following the emperor's victory the government was lodged in the hands of conservative Catholic patricians, and in 1555 parity was established in religion but not in politics, from the exercise of which the Lutherans were excluded. Their ecclesiastical ordinance of 1572 was a unique example in Germany of an evangelical church organization independent of political authority.

Seubert's little monograph tells all this in efficient prose but offers little beyond the recital of familiar events. The "investigations" of his title are nothing more than descriptions of incidents in the Reformation in Dinkelsbühl. At the most interesting junctures in the story (the active support given to the besieging peasants by substantial numbers of burghers, reasons for religious choice among citizens, the impact on society of the new marriage court, etc.) the author avoids analysis or confesses inability to explain. Beyond a certain usefulness as an outline of events and a handy reference to the sources, the book seems to serve no real purpose.

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CARLA KRAMER-SCHLETTE. *Vier augsburger Chronisten der Reformationszeit: Die Behandlung und Deutung der Zeitgeschichte bei Clemens Sender, Wilhelm Rem, Georg Preu und Paul Hektor Mair.* (Historische Studien, Number 421.) Lübeck: Matthiesen Verlag. 1970. Pp. 95. DM 18.

Carla Kramer-Schlette presents a highly schematic, comparative study of four Augsburg chroniclers living at the outbreak of the Reformation. The work is conceived first in terms of the chroniclers' presentation of objective material bearing on the social, political, and constitutional relations of the city and, second, in terms of the subjective cultural worth of the sources according to the consciousness of the authors. Whatever historical value the chronicles possess lies in the latter realm.

With a single exception there are no surprises here. The chroniclers ignore motives and context. One seldom obtains a glimpse of the internal power structure and its relationships.

Even the new religious ceremonies and doctrines, although supported by all but Clemens Sender, are reported vaguely. Our questions are obviously not theirs. Their nominalistic perception usually fails to penetrate beyond the externals of an event: for Georg Preu the *reformation* is principally the destruction of idols. Occasional vignettes emerge: the aged Emperor Maximilian on his knees at mass; Charles V, bare headed, in the Corpus Christi procession; the description of a brutal execution. It is still the world of Huizinga. And yet the relieving influences of humanism are curiously absent. The chronicles ignore Augsburg's culture and Conrad Peutinger is only mentioned to be attacked for self-serving. Preu, himself a painter, delights in the iconoclasm. In the bibliography of the monograph one notes the absence of Paul Joachimsen's study of Sender's predecessor, the Benedictine monk-humanist and Augsburg chronicler Sigismund Meisterlin, as well as Joachimsen's great work on humanism's impact upon German Renaissance historiography. The problem of humanism's presence or absence at least warrants formulation. Excellent in its close analysis, the monograph yet suffers from being too narrowly conceived.

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GERHARD MASUR. *Imperial Berlin.* New York: Basic Books. 1970. Pp. vii, 353. \$7.95.

Of all the capitals of modern Europe only Berlin leaped suddenly to the pinnacle of fame and power. A disunited Germany had known many cities of eminence and cosmopolitan elegance; Berlin was not always one of them. In the 1850s Berlin's economic growth quickened, and after 1871 her new place as the capital of Imperial Germany sealed her importance in the world.

Berlin was a superb example of the old and the new existing side by side; the court remained, the bourgeois plutocrats arrived, as did tens of thousands of immigrants from the country seeking employment. Jews struggled to establish themselves in the wake of full emancipation. Here was a city of conflicts and problems, of cultural ambition and garish ostenta-

tion. Here is a topic that might attract historians of different genres: a modern historian would explain this strange amalgam from a new perspective and with new techniques, while an older type of historian might try to recreate the atmosphere of Berlin, to sketch the conditions of society and the rise of its artistic and intellectual life. We need studies of all kinds.

One would have expected Professor Masur, who spent his early years and university days in Berlin, to give us an elegant version of the older type of history. His work, however, is marked by such serious faults of organization, substance, and style that it possesses few of the virtues and many of the weaknesses of this earlier genre.

Professor Masur has written a short book based on published sources; he did not turn to archival or statistical material. He endeavors to cover all facets of Berlin's life, and his book is a useful introduction to the city's economy, society, and high culture. He offers a kind of generalized Baedeker, but his scholarly aim and method are not clear. "This is not an economic history of Berlin," he writes, but neither is it a social history of the city, for it says little about how different classes lived, worked, played, or thought. He makes the important assertion that "in the second empire Berlin became an a-religious city," but he does not elaborate on this and in this apologetic epilogue merely reiterates his assertion and contends "that in consequence that phase of Berlin life could be ignored without loss." Can it? It is an *aperçu* that should have been bolstered by fact and illustration and enriched by reflection. Even the Youth Movement—born in Berlin—is dealt with in one inadequate paragraph. Much space is devoted to a conventional summary of German political history, especially during the Great War. Masur's judgments in this realm lack freshness and imagination. To conclude, for example, that "it is difficult to see that Ebert had a choice unless he had wanted to become another Lenin," is to be content with Ebert's own perspective on his predicament. Have we learned nothing in the intervening half century?

As one would expect, Professor Masur, the author of *The Prophets of Yesterday* (1961), dwells on Berlin's varied cultural and artistic

life. As in the rest of the book there is much here that is informative and pleasantly anecdotal. Theodor Fontane certainly was the most interesting Berlin writer of the time, but Masur's fifteen pages on him constitute a mixture of biography and plot analysis not an interpretative essay on Fontane's role and work. It is surprising that someone so familiar with Ranke would have devoted but one—conventional—page to him. Finally the reader misses a treatment of Berlin's popular culture. What did the different groups or classes read? The book is consistently idiosyncratic without being consistently interesting.

*Imperial Berlin* is written with ease, but occasionally an imprecise phrase or stylistic lapse ("the emotional subjectivism of the romantic movement now somersaulted and embraced the collective order") mars the text. The best passages of this book are anecdotal and autobiographical, and one regrets that Professor Masur did not choose to write the recollections of a cultured, upper-class Berliner; perhaps he could have done for Imperial Berlin what Stefan Zweig in *The World of Yesterday* (1943) did so poignantly for prewar Vienna.

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GIUSEPPE TALAMO. *La formazione politica di Agostino Depretis*. (L'età del Risorgimento. Studi e testi, Number 7.) Milan: Dott. A. Giuffrè Editore. 1970. Pp. 246. L. 2,400.

Until recently Italian historians have rarely regarded the writing of biography as an important scholarly endeavor, and this work by Giuseppe Talamo gives cause to fear that the art may not yet be understood in Italy. The subject, Agostino Depretis, virtuoso and founder of the practice of *trasformismo*, overshadowed an entire decade of Italian history. Much is known of his parliamentary dictatorship from 1876 to 1887, but little has been written of the preceding sixty years of his life. Talamo seeks to fill this gap, but the result falls far short of the goal.

This is a set of five crudely linked and badly structured essays, each with appended documents, on as many segments of Depretis's life and family background that carry us to the turn of the century when he was thirty-eight years old. The first study is on the establish-

ment of the Depretis family (Agostino and his father Francesco) as landowners of growing wealth, and the second purports to treat Agostino's administration of the lands of the wealthy Piedmontese family of the Gazzaniga Arnaboldi. Essentially what emerges is that Depretis was of Lombard origin, not Piedmontese as has long been believed, and that in 1843 he conceived a modern scheme for the management of the Gazzaniga holdings. But these are needles to be dug from Talamo's haystack. The author has presented information germane to the formation of the future statesman, but it is in the form of ponderous, raw statistical material, the type of data readout one would get from a computer programed to report on the family's economic and business activities. Such information is little more than a tiny insight into the minutiae of agrarian economy in one small region; on the future prime minister we have precious little. While Depretis's birth registry is faithfully reproduced in the original Latin, he appears again only as an adult, an illustration of the author's tendency to omit biographical information. Why have we no account of his formative years? Not one word has Talamo provided on Depretis's education, or *cultura*, something Italians have always regarded as a *deus ex machina* in their leaders. Was he indeed a lawyer as is generally believed? If so, more than incidental mention of the fact is crucial to his political formation.

In the next two essays on Depretis in Parliament in 1848-49 and as mayor of Stradella in 1849-50, Talamo veers a bit closer to his subject. We are told, almost incidentally, that Depretis's experiences in the decade prior to 1848 slowly matured him and that he fought for broader application and more liberal interpretation of the *Statuto* in the Kingdom of Piedmont, that he was a democrat. In short Depretis is quite clearly delineated in his Left convictions of the moment, but Talamo concludes here with a tantalizing mention of the *connubio* and nothing more. Hence, apparently on the threshold of allusion to Depretis's reactions as a deputy to that first form of *trasformismo*, the author leaves his reader to speculate.

The chapter on Depretis as mayor of Stradella is actually a three-part essay in which only the last section deals directly with the subject. The remainder is, first, a socioeconomic

study of the town with pedantic lists of its "zootechnical patrimony" and, second, a brief account of Depretis as city councilman. The material is neither uninteresting nor unimportant, but Talamo's handling of it lacks coherent narrative.

The last essay treats the founding and brief life of the *Progresso*, a Turin newspaper in which Depretis was actively interested in 1850-51. Here again Talamo knits ideas together a bit better, but he fails to tell much that is not already known about an obscure subject. The *Progresso* had links with Mazzini, we learn, but so did various other contemporary periodicals. Would we not be more aided by a study of Depretis the publicist that is broad enough to include also his work with *La Concordia* in 1848 and after?

Talamo's work is excessively fragmentary; depth studies of moments do not elucidate decades. The writing is frequently turgid, and the author often yields to trivia and cannot resist engulfing his reader with oppressive documentation that hinders an already leaden text. Often he drifts into digressions from his already inappropriate title, and there is one supporting document of no relevance (a memoir on agricultural conditions [pp. 70-76] in Depretis's home region dated 1879, more than thirty years beyond the most recent reference to the subject).

Making fair allowance, as indeed we must, for the author's promise in his preface of a later evaluation of this material, the volume still remains a frustration. It is frustrating because so much painstaking research has gone into the task, but it smacks of the assiduousness found occasionally in an industrious but unimaginative graduate student. Talamo has taken a figure who still appears stodgy in Italian history, and so he has left him. Even if Depretis did govern in a period of "dry-as-dust," his stature was such that someone should be able to bring him alive. Yet Talamo leaves all this to the reader's fantasy.

In his introduction the author notes that parts of his book originally appeared in scholarly journals, but he does not inform us that chapters 1, 2, and 5 were first published as *dispenze* or university lectures (*La formazione di Agostino Depretis* [1968]). Indeed what is wrong with this book is that it maintains the



classroom format. Comparison with that first edition is sadly enlightening, for little has been changed beyond an occasional word or, worse, paragraphs and entire pages shifted from one chapter to another. At least the addition of an adjective to the book's title clarifies matters slightly, but all remains raw and pedantic. Yet Talamo is listed as the author of a forthcoming biography of Depretis in the *Unione Tipografico-Editrice* Torinese series. Perhaps, as the subtitle of the 1968 edition suggests, this is nothing more than notes for a biography. If Talamo's research continues in this depth, a polished complete biography might be outstanding; if he publishes it in the present form, one volume will not suffice and Depretis will still await his biographer.

Another aspect of this work is ironic and tragic. Produced as lectures at the University of Rome in the disastrous year 1967-68 and required for examinations in *Risorgimento* history, these pages are a document that illustrates and perhaps justifies complaints of students rebelling against the present curriculum.

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LUCIO AVAGLIANO. *Alessandro Rossi e le origini dell'Italia industriale*. (Università degli Studi di Salerno. Collana di studi e testi, Number 2.) Naples: Libreria Scientifica Editrice. 1970. Pp. 554. L. 6,000.

Alessandro Rossi (1819-98) was the founder of one of Italy's greatest establishments of the nineteenth century, the woolen manufactory of Lane-Rossi, which employed on the eve of the twentieth century over five thousand persons. Only a small portion of this volume is, however, devoted to a business history of this famous concern or to the early industrialization of Italy. Consequently in the scant fifty pages devoted to the business, attention to economic questions is both sketchy and elementary. To an economic historian this is a pity, the more so because the author apparently had access to the pertinent information, some of which he put in that half of the work given over to documents and tables.

Mr. Avagliano was clearly more interested in the public career of Rossi than in his business affairs, and his choice of emphasis is under-

standable. Rossi was one of those early entrepreneurs who, like Robert Owen, was deeply concerned with the welfare of his workers and of society in general. Thus one is not surprised to find that he was an exponent of a guild organization of the state, Catholic social welfare movements, model housing for his workers, the education of youth, and mutual aid societies. In politics he was at first a Cavourian liberal who opposed the old *Destra*, but he later supported *Crispi* and worked for the protective tariff of 1887. He was active in local politics at Padua, served as a deputy, and was then appointed to the Senate.

This biography touches on a multitude of questions that confronted Italy in the last third of the nineteenth century, but it does not take a hard, clear look at any of them. Its virtues are that it has a great amount of detail, which will make it valuable as a reference book, and a wealth of leads to business archives.

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C. DAICOVICIU *et al.*, editors. *Acta Musei Napocensis*. Volume 7. (Comitetul de Stat pentru Cultură și Artă.) Cluj: Muzeul de Istorie. 1970. Pp. xvi, 668.

This most recent collection of writings on Transylvanian history by the scholars of Cluj is, like previous volumes in the series, weighted toward the pre-Roman and Roman periods and various aspects of the national movement between the latter part of the eighteenth century and the end of the First World War. This emphasis reflects, on the one hand, both the interests of the editor, the distinguished classical archeologist and historian, Constantin Daicoviciu, and the intensive study of ancient history at Cluj and, on the other, the increased nationalist orientation of Romanian historiography in the last decade. Only brief mention may be made here of a few of these and other articles.

In the first group, Valentin Vasiliev describes pieces of jewelry discovered recently in Scythian graves that suggest the need for a fresh look at the problem of the Scythians in Transylvania; Iudit Winkler describes the chronology and types of the earliest Geto-Dacian coinage; Hadrian Daicoviciu makes a number of interesting observations on Trajan's

first war with the Dacians; and I. I. Rusu continues his series of "epigraphical notes" on Greek and Roman inscriptions. In the second group, Aurelia Bunea presents a well-documented study of how the Romanian question in Hungary was treated in the Romanian parliament between 1892 and 1899. She concludes that the nationality policy of the Hungarian government helped to turn Romania away from the Triple Alliance toward the Entente. Acațiu Egyed has written a brief but enlightening article on the emigration of peasants from Transylvania between 1900 and 1913. He analyzes their social status, reasons for leaving, destinations—the great majority went to the United States—and the effects on Transylvania of this sizable exodus. Finally, Dumitru Suciu discusses critically the attitude of the French press toward the union of Moldavia and Wallachia in the period from 1856 to 1859.

There are, in addition, two short but important articles dealing with the period of the *Völkerwanderung*: a description by Kurt Horedt and Dumitru Protase of a late fifth-century treasure discovered near Cluj that may be Ostrogothic rather than Gepid and may, therefore, necessitate a reassessment of Germanic settlements in the area and an analysis by Ștefan Ferenczi of two jars dating from the end of the sixth or the beginning of the seventh century that may be the oldest traces yet discovered of Slavic settlement in Transylvania. Mention must also be made of Magdalena Bunta's extremely interesting and original contribution to the history of the Anabaptists in Transylvania in the seventeenth and eighteenth centuries and Marius Porumb's study of selected sixteenth-century icons in Maramureș and northern Transylvania.

The volume also gives some attention to more recent historical developments, but in these articles the influence of current political trends is clearly evident. Gheorghe Bodea, for example, provides much useful information on the establishment of a democratic regime in northern Transylvania in 1944-45, but he is mainly concerned with stressing the leading role of the Communist party and the cooperation of Magyars and Romanians. The result is, inevitably, a one-sided piece that ignores diversity of opinion and controversy and never gets

to the heart of the age-old nationality problem in Transylvania.

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DAVID JORAVSKY. *The Lysenko Affair*. (Russian Research Center Studies, Number 61.) Cambridge, Mass.: Harvard University Press. 1970. Pp. xiii, 459. \$13.95.

If one wonders why this study by a historian on the genetics controversy is reviewed by a political scientist in a historical journal, the answer is quite simple: the Lysenko theories, purported to be genuine, turned out to be a quack science supported primarily by Soviet authoritarian politics that, in turn, have been subject to historical inquiry.

American scholars who study closed societies are always searching for cracks or holes in the political walls through which they might observe and analyze political cultures manipulated by authoritarian elites. The Soviet Union has such a society, and Professor David Joravsky is an American scholar who found a "hole," the Lysenko affair, through which he chose to observe and analyze "the evolving interaction of agriculture, natural science, ideology, and political power" in the Soviet Union. The prime rule of this case study, according to Joravsky, "is to determine the political unknown by its interaction with strategically selected knowns, as they are evidenced in the public record." Joravsky did so, and as a result he tried to answer two basic questions: "How can we distinguish Soviet ideology from other types of thought, and, how can we be precise and factual in analyzing ideological influences in Soviet history?"

Unlike some of the earlier writers on this subject (for example, J. S. Huxley, Conway Zirkle, and John Langdon-Davies) Joravsky tries to refute the allegation that Lysenkoism was a direct outgrowth of Marxist theory in the 1930s. According to Joravsky, "the most well-known Lysenkoite writings lay overwhelming stress on agricultural practice as the chief source." And since the criterion of practice is the chief principle of Soviet ideology, Joravsky equates Lysenkoism with Soviet ideology rather than with Marxist theory. While it is plausible

to argue that Soviet ideology was and still is geared toward "the criterion of practice," it is equally justifiable to claim (as Joravsky does) that Soviet agriculture suffered for thirty-five years because of Lysenkoite practices. If so, why did the Soviet leaders, with the exception of Brezhnev, permit this setback in Soviet agriculture? The answer, at least to me, is obvious. When the Lysenko controversies reached a high plateau in Soviet scientific circles, the issues involved in the controversy were so close to the sensitive ideological pole of communist dogma that it was imperative for the Soviet elite to render interpretations based on Bolshevik morals. The question raised by the issues was not which theories would lead to greater agricultural output in the Soviet Union but which theories were more compatible with the dogma when applied to agriculture and genetics. In the final analysis, the issues were reduced to Soviet socialist science versus "decadent bourgeois science." Of course, as in many other cases, the interpretation rendered by the party was consistent with Soviet hypocrisy. While many Soviet agricultural scientists were paying lip service to Lysenkoite theories, at the same time they were practicing "bourgeois science" in agriculture and genetics. Soviet leaders tolerated this situation because the increased yield of corn, wheat, and other crops in such areas satisfied their ideological interests.

The creator of "agrobiology," Lysenko persuaded Stalin and his successors to accept his approach to Soviet agriculture because it could render the desired increases in farm yields at little or no cost. Thus, from 1929 to 1964, with a few ups and downs, Lysenko had the support of the party and its auxiliaries to impose his "scientific methods" not only on collectivized peasants but also on scientists, first in the preparation of seeds and in the science of plant physiology, then in plant breeding and genetics, and finally in a wide range of agricultural techniques and related sciences—from afforestation to the use of fertilizers, from cytology to soil science. As a result the damage to Soviet agricultural science was enormous. This aspect of Lysenkoism, however, is only of secondary interest to Professor Joravsky. His primary concern is the political repercussions that followed the Lysenko controversies under Stalin and Khrushchev. Therefore, he examines the volu-

minous public record that abounds with evidence of change in policies but offers little insight into the secrecy of high Soviet politics. It is perhaps for this reason that Professor Joravsky does not contribute much to our knowledge of the period of the struggle for power after Stalin's death. Although he cites the V. S. Dmitriev case, its political significance is not fully explained. Nor does he explain the criticism of Lysenko launched in 1954 by Malenkov's protégé, Khrushchev. Professor Joravsky claims that from the end of 1954 until his ouster in 1964, Khrushchev had supported Lysenko in all his endeavors. The evidence, however, suggests that it was not until December 1958 when Khrushchev, *Pravda*, the presidium of the Soviet Academy of Sciences, and *Izvestia* assailed the editors of the *Botanicheskii zhurnal* for criticizing Lysenko. From 1952 until 1958 Lysenko was systematically refuted by the editors of the journal. Although we have no proof that such criticism was instigated by N. S. Khrushchev, we do know that he did not stop this criticism until 1958.

From a historical point of view, one can draw at least two valuable lessons from Joravsky's study of Lysenkoism. One is that there has been a tendency in this country to oversimplify the relationship of Soviet science to Soviet ideology. In this respect Lysenko did almost as much harm to American space science as to Soviet biology, for he encouraged skepticism in the United States about scientific achievements in the Soviet Union. A second point is that the basic task of Soviet science is to facilitate the universal development of the productive forces of the country. It is a dutiful and fully subordinated tool of the elite in power. Thus, when the Soviet leaders felt the need for the services of the scientists, they were willing to rehabilitate those having committed "treason."

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#### NEAR EAST

G. E. VON GRUNEBaum, editor. *Theology and Law in Islam*. (Second Giorgio Levi Della Vida Biennial Conference, May 9-10, 1969, Near Eastern Center, University of California, Los Angeles. Published under the auspices of the Near Eastern Center, University of California, Los Angeles.) Wiesbaden: Otto Harrassowitz. 1971. Pp. 105. DM 48.

The occasion for *Theology and Law in Islam* was a conference held in May 1969 to mark the bestowal of the Giorgio Levi Della Vida medal upon the late Joseph Schacht, and the six papers published here are the proceedings of that conference: Schacht himself on "Theology and Law in Islam" (pp. 3-24); W. Montgomery Watt, "The Great Community and the Sects" (pp. 25-36); Walther Braune, "Historical Consciousness in Islam" (pp. 37-42); Gerhart Ladner, "Reform: Innovation and Tradition in Medieval Christendom" (pp. 53-74); George Makdisi, "Law and Traditionalism in the Institutions of Learning of Medieval Islam" (pp. 75-88); and Fazlur Rahman, "Functional Interdependence of Law and Theology" (pp. 89-97).

The papers of Schacht and Makdisi taken together summarize the interrelationship between jurisprudence and Islam's dialectical theology (*kalam*) from the beginnings down to the twelfth century, and it is immediately apparent that comparisons are difficult. Jurisprudence reached maturity in the early ninth century when *kalam* was approaching adolescence. It was the law, too, that dominated Muslim institutions of higher learning, and theology came into them, as Makdisi points out, by a side entrance—through the academic sermon. In this connection Makdisi gives an instructive summary (pp. 77-79) of the fundamental differences between the Muslim *madrasah* and the European university.

Rahman's approach to the same question is not so much historical as ideological. Jurisprudence is a science of the particular act and *kalam* of the universal principle. There never developed between them, Rahman regrets, a system of moral philosophy. In my opinion, the conclusion appears to render something less than justice to both Muhasibi and Ghazali.

Watt is interested in the heresiographies and their presuppositions, and the greater part of his paper is given over to tracing the history of some of the names put upon various sects, generally by their opponents but occasionally, as in the case of the Shi'ites, by the partisans themselves. The example of the Shi'ah, though offered only in passing, is particularly interesting, but it is the only place in the volume where the issue comes up. The notorious problems of Shi'ite theology and Shi'ite traditions

have been addressed by very few Western scholars, and that unhappy fact is painfully illustrated in this collection.

One of Watt's theses is that Western scholars look for "progress" in the matter of intellectual history while the Muslim more often finds reprehensible "innovation." This may be true of the traditionalist Muslim lawyer, but Braune attempts to nuance Islam's view of history by adducing the contrasting attitudes of the pre-Islamic poets, the Prophet, the Mu'tazilites, and the Sufis. Braune makes the case that the Mu'tazilites were Islam's chief apostles of man's historical self-realization and consequent reform, while Sufism took men out of history.

Ladner's portrait of medieval Christendom is quite different, with the monastic orders in the van of reform movements. Ladner does not make a point of them, but the differences are provocative for an Islamicist, and it is to be regretted that the editor, as so often in the past, did not in this instance publish the participants' comments on each other's papers.

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GEORGES I. BRATIANU. *La mer Noire: Des origines à la conquête ottomane*. (Acta Historica, Number 9.) Munich: Societas Academica Dacoromana. 1969. Pp. 394. DM 38 (\$9.50).

This volume, written some two decades ago by Professor Bratianu and now published for the first time, represents an important addition to our understanding of the Black Sea during ancient and medieval times. It also makes us regret even more the tragic end of its author after he fell into the hands of his political opponents following World War II, when he was still in his prime.

The book itself is divided into four parts or sections. The first begins with a description of the special physical and geographical characteristics of the Black Sea and the areas that surround it and then surveys its history in ancient times until the end of the early Roman Empire. The second portion deals with this region from the third through the twelfth centuries A.D.—which might be called a late Roman-Byzantine age. The third section concerns itself with the years between the time of the Fourth Crusade and the mid-fourteenth century, which the author calls a Mongol-Italian period and

with which he has dealt extensively in an earlier book. The last portion tells the story of the two centuries between 1300 and 1500 A.D., which saw an intensive rivalry for Black Sea mastery between Venice and Genoa and the first conquest of the sea and its shores by the Ottoman Turks.

As one would expect from a knowledge of the author's earlier work, there is a heavy emphasis upon social and economic factors in the history he relates, although political developments are not slighted. It is also worth noting that this remarkable work of synthesis is placed in a world setting that relates events taking place in the Black Sea region with those affecting the wider world of Eurasia and Africa. One is also struck by the breadth of Bratianu's knowledge and understanding of the historical process and the wide range of his scholarship, which makes this book as fresh and timely as if it were written yesterday.

In short here is historical synthesis at its best, beautifully written by a scholar who is able to explain his own chosen area of Europe and Asia so that it not only throws light upon other regions during ancient and medieval times, but at the same time deepens our understanding of history in general.

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MICHAEL CURTIS, editor. *People and Politics in the Middle East*. (Proceedings of the Annual Conference of the American Academic Association for Peace in the Middle East.) New Brunswick: transaction Books; distrib. by E. P. Dutton, New York. 1971. Pp. 325. Cloth \$8.95, paper \$3.95.

The motivation of this volume and of the conference at which most of the papers were first presented is the belief that analysis of various aspects of the Arab-Israeli conflict will contribute to its peaceful resolution. Some of the contributors approach the objective somewhat indirectly. S. G. Haim's contribution is a useful summary of recently available Arab information concerning the origin of the Ba'th party. Two studies, one by H. H. Smythe and Sandra Weintraub and the other by Yochanan Peres, treat ethnic and cultural groups in Israel. The former views the conflict between

Orientals and Europeans more seriously than does the latter. The Peres study uses survey data to deal with sophisticated and important theoretical questions. Eliyahu Kanovsky challenges most current estimates of recent economic developments in Egypt, Israel, and especially Jordan.

The approach to the objective is more direct in the remaining eleven essays and in the accounts of the three discussion periods. Several subjects recur throughout, even in essays whose chief concern lies elsewhere. Doubts of the Palestinians' claim to be an entity are raised by Ben Halpern (whose chief thesis is that Israelis and Arabs have borrowed each other's ethical arguments), Marie Syrkin, Samuel Merlin, and Yehoshafat Harkabi. Their aim is to deny the need of the Palestinian Arabs for all of former Palestine—that is, the elimination of Israel and the reconstitution of Palestine—in order to realize their national existence, since the Palestinians, as Arabs, can enjoy national existence in a large Arab territory. Don Peretz argues for the legitimacy of a Palestinian entity. We may add that the present insistence on the Pan-Arab nature of Palestinian nationalism seems strange in view of the former objection to the involvement of non-Palestinian Arabs in the Palestinian question. Furthermore, the Palestinians today are giving the same answer to the question of local patriotism and Pan-Arabism, which are not regarded as contradictory, that other Pan-Arabs did from the 1920s on. Syrkin and Merlin, as well as Peretz, believe a "Palestinian State," that is, the Jordanian west bank, preferably with the addition of the east bank, can facilitate the ultimate settlement. Most of the contributors with Israeli sympathies believe that Arab refusal to recognize the existence of Israel is independent of Israeli policy or action. The exceptions are Peretz and even more Merlin, who is critical of aspects of Israeli internal politics and argues that Israel cannot claim to be willing to negotiate with the Arabs in good faith and without preconditions. Jon Kimche thinks, erroneously in my opinion, that the British from 1918 on deliberately prevented Arab-Jewish cooperation. Amos Perlmutter offers an up-to-date version of the familiar criticisms of Dulles and American cold warriors in general. He is often penetrating when dealing



with particulars, but his treatment suffers from the usual combination of snippets of historical information with an interpretation that is no more convincing than was the theory of the cold war social scientists he criticizes. Shlomo Avineri explains how New Left critics can be won over to Israel by showing that Israel, not the Palestinian Arabs, is the analogue of North Vietnam. Peretz probably would disagree. Avineri's discussion ignores the vital effect of British military force on the relative positions of the Jewish and Arab communities in Palestine. In any event Arab, not Israeli, membership in the universal church of peoples' national liberation has been certified by the prelates—the bishops of Havana, Peking, and Hanoi.

Representing Arab views, Abdul Aziz Said believes that the post-1967 Palestinian movement is a new and truly revolutionary phase in Arab history, and F. J. Khouri argues that Israel could agree to the return of the Palestinians without endangering its security. F. H. Hinsley believes, with good reason, that only the powers can bring about a settlement of the Arab-Israeli problem and, with less apparent reason, that there is an emerging agreement among the powers.

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R. HRAIR DEKMEJIAN. *Egypt under Nasser: A Study in Political Dynamics*. Albany: State University of New York Press. 1971. Pp. xvi, 368. \$10.00.

At first glance this bulky volume may appear to be a rather forbidding exercise in the application of concepts from sociology and political science like the "routinization" and "legitimation" of the charismatic leader's rule. But the reader who perseveres soon finds that Professor Dekmejian has written a lucid and readable analysis of interest to many besides the professional social scientist. Drawing both on Arabic materials and on studies by Western scholars, he exposes the shallowness of supposing that Gamal Abdel Nasser was a second-rate fascist dictator or the dupe of the Kremlin. He argues persuasively that Nasser was the greatest Arab hero since Saladin and Muhammad and that

his popularity among Egyptians survived almost unscarred a long list of failures, from the defeat by Israel in the Suez War of 1956, through the secession of Syria from the United Arab Republic in 1961 and the disastrous involvement in Yemen's civil war ("Egypt's Vietnam"), down to the Six Day War of 1967. Nasser's offer to resign at the close of that humiliating conflict was rejected by acclamation, and his death three years later was the occasion for mass mourning of an intensity matched in recent history only by Indian grief for Gandhi and Nehru.

How are we to account for this extraordinary record of nothing succeeding like the absence of success? Because, Professor Dekmejian contends, Nasser endowed the abstractions of Arab nationalism with a new psychological reality and still more because his reiterated stress on the need for dignity gave the Egyptian peasant a chance to identify with the Nasser government in contrast to his traditional hostility toward his rulers. Yet, as the author also observes, "charisma can only inspire men, it cannot organize them" (p. 250). He argues that the year 1961 marked the watershed of Nasser's domestic policy, as the Syrian secession led him to complete the organization of a sweeping program of "Arab socialism," which owed as much to Islamic traditions and democratic European examples as it did to Marxism. At the same time more determined efforts were made to create a viable mass party, the Arab Socialist Union. Its vicissitudes and the shifting roles of the military and academic elite in Egyptian cabinets (many ministers had Ph.D.'s) are followed in considerable and informative detail.

Throughout the book Professor Dekmejian (who is of Armenian stock) is no uncritical hero-worshipper. He notes, for example, that Nasser made General Neguib an "unperson" as soon as he had maneuvered him out of the presidency in 1954. And he blames the fiasco of the Six Day War in part on the backfiring of Egyptian "brinkmanship" based on Nasser's underestimation of the intensity of the Israelis' nationalism and their disposition to seize the boldest of the options open to them.

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## AFRICA

J. DESMOND CLARK. *The Prehistory of Africa*. (Ancient Peoples and Places, Volume 72.) New York: Praeger Publishers, 1970. Pp. 302. Cloth \$10.00, paper \$3.95.

Since the publication of H. Alimen's *The Prehistory of Africa*, translated by A. H. Broderick (London, 1957), archeologists have taken great strides in Africa. A new survey was overdue. But this book is not merely a survey brought up to date; it is a work that shows a bold change in conception over all former textbooks. No longer is this a record of man's tools but a history that weaves the physical and cultural evolution of mankind into a single story. Hardly anyone but Desmond Clark could have succeeded in this venture. And no story could be more stirring than man's emergence from the apes and his slow but accelerating evolution ever since.

The book opens with a chapter on methodology and then follows the main chronological sequences: the australopithecine and *Homo habilis*; *Homo erectus* (the unspecialized hunter); modern man (*Homo neanderthalensis*, *Homo rhodesiensis*, *Homo sapiens*); the specialized hunting and gathering societies; the farmers and present-day people. The most crucial step in man's evolution is the emergence of modern man to whom the author attributes the invention of speech and ritual and the discovery of fire between 70,000 and 35,000 years ago.

One admires Clark's virtuosity and breadth of knowledge that cover many disciplines in the natural, physical, and social sciences. Here and there are some weaknesses—in linguistics, perhaps, or in psychology—but even so there are no real lapses. Truly *Homo mirabilis* wrote this account!

For the earlier periods a few remarks must suffice since the historian is most interested in the last chapter of this book. The first warning must remain that this is a case where there are too few skeletons in the closet, especially when one deals with modern man and later ages. Human remains are too few to allow us to term as "racial" the subtle variations found. These variations are not gross anatomical differences as they were during the early evolution of man. They can fall well within the range of variability of modern populations.

Also, there are not yet enough excavations to feel confident at all points that the distribution maps given will not undergo major changes as time goes by. A splendid example is the case of the channeled and dimple-based potteries associated with the coming of iron to Equatorial, East, and Southern Africa. Since this book was written, there has been at least one stylistic study that shows a dozen subvarieties in this "co-tradition." But more important, sites are now known on the lower Congo, on Lake Leopold II, and at least a few shards have come from Kinshasa so that the whole distribution is altered. At all times, therefore, the reader must remember that Africa's archeological exploration is still in its infancy or adolescence. He must also remember Clark's own words in his first chapter about the need for interpretation of data and therefore the possible fallibility of some interpretations.

The last chapter deals with the initial stages of food production, the spread of domestication (especially in East Africa), the spread of the Bantu, metallurgy, and Iron Age societies. One cannot agree with the statement on page 193 that pharaonic government is very reminiscent of the rule of the despots of later times on the upper Nile and in West Africa. This is in a moderate form a revival of the "Sudanese kingship" theory, which will not hold water. The pharaohs remind one just as much of Javanese monarchies or early Indian states.

Attention is drawn particularly to Clark's treatment of the early date for cattle in the Sahara and to his ingenious proposition that this may indicate either independent domestication in North Africa or, perhaps more likely, an introduction by sea from Italy to Tunisia by the middle of the fifth millennium. Only further data will allow us to decide whether either of these two theories is correct or whether we should stick to the theory of diffusion from the Middle East.

On the spread of agriculture a few major points must be mentioned. First, of all the domesticated crops (see the map on page 212 and the table on page 213), only the sorghums and millets domesticated in the eastern Sudan or in Ethiopia spread into East, Equatorial, or Southern Africa. This virtually implies that agriculture had spread before the Bantu came. The date for the spread from Ethiopia to

Kenya (p. 208) is put at the end of the first millennium B.C., but this assumption really rests on a single carbon-14 data, which may well (thirty-three per cent probability) be wrong. To me it seems wrong because we find that by 1800 B.C. African millet had already reached India, from Ethiopia presumably. So one should not be surprised if new sites show that animal domestication and agriculture reached East Africa much earlier than 1000 B.C.—in fact not long after 2000 B.C. In this whole discussion one might as well make abstraction of all the physical anthropology that is correlated with cultural movement simply because we do not have enough skulls and bones to be certain that we are dealing with different populations, “newcomers,” “blends,” or simply normal variation within a single population. On the same general topic the date assigned (p. 210) to Lanet by its excavators (the middle of the sixteenth century A.D.) can also be a freak, and it looks like one. But one must remember that linguistics, physical anthropology, and the study of culture show that in the first millennium B.C., the first millennium A.D., and later very complex human movements have taken place in East Africa as well. That puzzle will not be unraveled in the near future.

The author shows convincingly (pp. 205–06) that agriculture was almost certainly practiced in the southern Congo at an early date, before the coming of the Bantu. This fact must be accepted, and every interpretation of the processes that led to the spread of Bantu-speaking people and the domination of their languages over all others in the African “peninsula” must take this fact into account along with the probability that Bantu-speaking people may have moved before iron had spread among them.

The description of the spread of the Bantu is merely a rephrasing of Malcolm Guthrie’s hypothesis. It is not more than a hypothesis. Many linguists, while admiring much of Guthrie’s monumental work, disagree with his historical implications.

By and large one agrees with Clark’s view about the spread of iron in Africa. Perhaps, however, iron reached East Africa first on board the ships of south Arabian traders; and it reached Daima (p. 215), not from Meroë, but from the bend of the Niger after having crossed the Sahara. Graham Connah feels that

this is the only likely explanation for the sites in northern Nigeria (see, for example, Thurstan Shaw, *Lectures on Nigerian Prehistory and Archaeology* [Ibadan, 1969], 33). Since the Iron Age civilizations belong to the historical record in many cases, the short summary given here seems very general indeed to most historians and needs no discussion.

When all is read, one can only admire the book and learn from it. It will influence and even shape any interpretation in African archaeology for years to come. Some dates may be wrong, some assertions may become obsolete, but the approach will endure. That is what makes a great book.

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ROBERT I. ROTBERG. *Joseph Thomson and the Exploration of Africa*. New York: Oxford University Press. 1971. Pp. 360. \$10.00.

ROBERT I. ROTBERG, editor. *Africa and Its Explorers: Motives, Methods, and Impact*. Cambridge, Mass.: Harvard University Press. 1970. Pp. 351. \$12.95.

One cannot often review two books with such unreserved pleasure. By virtue of Professor Rotberg’s sensitivity and his brisk but restrained style, *Joseph Thomson* is rescued from both the tedium and the hyperbole often encountered in tales of trial and tribulation in darkest Africa and is transformed into one of those infrequent biographies that can be read for sheer enjoyment, should one’s interests happen to go no further. Rotberg, like Thomson, knows how and when to be “cool in print.”

Thomson’s portrait, however, is not altogether endearing. His approach to Africa was essentially romantic, his observations were often naive. He hungered for fame, could be prudish and hypocritical, was unstable and restive—a professional wanderer—and was plagued by fits of melancholy. His obsession with Africa drove him to illness and an early death. But despite his faults he is remembered for the sturdy gentleness, boundless goodwill, and gregariousness that got him through many a scrape and made the peaceful progress of his expeditions almost unique in African exploration. One of the book’s themes is Thomson’s conversion to Darwinian beliefs and imperial-

ism. Arriving in East Africa as an idealistic youngster prepared to admire and respect Africa on its own terms, he left the continent eleven years later convinced by observation, bad experience, and the fashionability of racial theories (and, one wonders, by the demoralizing effects of sickness?) that Africans spoiled by European contact could be redeemed only by long apprenticeship under benevolent European rule.

Thomson's achievements were limited. Coming as he did at the end of the era of exploration, only his first and third journeys were scientifically productive. Professing disdain for commercialism, three of his trips were nonetheless sponsored for commercial motives, while one can only be called recreational. A man of erratic insight, he realistically foresaw that Africa's trade potential was limited but assumed that the Kenya highlands were wholly unsuitable for white settlement. Always in a rush to cover the ground, his journals and published works proved to be disappointingly impressionistic and inaccurate. The total impression is that Thomson was perhaps the most important of the second rank of explorers.

The book is fully equipped with the proper scholarly paraphernalia: index, notes, a good bibliography of Thomsonia, and appendix noteworthy for the texts of Thomson's African trade treaties. Two technical negatives can be registered. The voluminous footnotes, parenthetically fascinating as they are, are somewhat distracting. The most significant should have been absorbed into the text, the rest discarded. Many place names found in the text are frustratingly missing from the maps.

*Africa and Its Explorers* is focused on nine men who, with one or two omissions, contributed most to African exploration and made the deepest public impression. The contributors were charged with a twofold task: to assess their subjects' historical contributions and to place them in an African context by a careful examination of all available indigenous accounts. The editor undertook a third task in the introduction: a comparative study of the explorers' motives and methods and of their individual responses to Africa and Africans. The outstanding merit of this collaborative effort is the achievement of the second task, of an African perspective that serves as a most useful

corrective to the prevailing Eurocentrism of studies of exploration. The book is a series of neat vignettes that furnish the reader with an *Überblick* of preimperial Africa through the eyes of the men on the spot, African as well as European.

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RUTH FIRST. *Power in Africa*. New York: Pantheon Books. 1970. Pp. xiii, 513. \$10.00.

JON WORONOFF. *Organizing African Unity*. Metuchen, N. J.: Scarecrow Press. 1970. Pp. x, 11-703. \$15.00.

T. PETER OMARI. *Kwame Nkrumah: The Anatomy of an African Dictatorship*. With a foreword by NII AMAA OLLENNU. New York: Africana Publishing Corporation. 1970. Pp. xix, 229. \$8.50.

PETER GEISMAR. *Fanon*. New York: Dial Press. 1971. Pp. 214. \$6.95.

The first two works, *Power in Africa* by Ruth First and *Organizing African Unity* by Jon Woronoff, are studies of Africa as a whole; while the latter two are studies of specific figures—a pioneer of the African revolution, Nkrumah, and a major philosopher of both the African and Afro-American revolutions.

In *Power in Africa* Ruth First has made a real contribution to African studies through her searching analysis of the role of the military in nation building among the African states. A statement in the introduction reveals the nature of the work: the author says, "This book is about army intervention in politics, but more about politics than armies." It is not, accordingly, a book about the mechanics of armies and coups d'état, their logistics and command structures, but about the way army interventions in politics reveal the nature of political power and its areas of failure in Africa.

The opening section of the book deals with the African political and economic situation in the period that followed independence. A brief but well-done analysis of the various problems of Africa is included. The role of the army in the new African states and the "contagion of the coup" are especially effective. The second section of the book entitled "The Colonial Sediment" is required reading for all who would

understand or attempt to understand independence in the African continent. The third, fourth, and fifth sections study the "successor states," the breakdown of authority, and the role of the coup. The author selects three nations as the focus of her study of the coups of Africa—Ghana, Nigeria, and the Sudan. The last portion of the work, and a most potent one, seeks an analysis of the army and its future role in nation building in Africa. *Power in Africa* is more than a summary of the various African coups; it is an effective presentation of the role of the military and its interrelation with other elements in the political and general life of the new African nations.

*Organizing African Unity* is a rather voluminous study of the Organization for African Unity. While some of its materials are duplicated in such works as V. T. Thompson, *Africa and Unity*, it nevertheless makes a contribution to the study of the African search for unity. It is regrettable that the format of the work leaves much to be desired in the matter of explanatory materials. The casual student of African studies would, I fear, experience difficulties in following the presentation of the materials and the views of the author.

T. Peter Omari, in his *Kwame Nkrumah: The Anatomy of an African Dictatorship* (1970), has added a valuable link in the understanding of Nkrumah. The student of African history, especially Ghanaian, now has available a rather extensive group of books—Nkrumah's own writings; A. A. Araf, *The Ghana Coup* (1966); John Phillips, *Kwame Nkrumah and the Future of Africa* (1960); Bob Fitch and Mary Oppenheimer, *Ghana: End of an Illusion* (1966); and Henry L. Bretton, *The Rise and Fall of Kwame Nkrumah* (1966).

Mr. Omari's introductory chapter seeks to present an overview of the Nkrumah period. While he presents Nkrumah's guilt, he finds that others, principally the judiciary, share the blame. The succeeding chapters trace the Nkrumah period; especially noteworthy is the chapter "The Many Sides of Nkrumah." Included in an appendix is a presentation of "Nkrumahism-African Socialism: Ghana's Conception of Socialism." This work adds a balanced account to a controversial period of African history.

The last work, *Fanon* by Peter Geismar, is

the latest effort to capture the mercurial personality of Frantz Fanon. While this study of Geismar's gives a real insight of Fanon's psychiatric theories, it fails to capture clearly Fanon as a philosopher. In all fairness to Mr. Geismar, the implications and nuances of Frantz Fanon will never be easy to capture. A bibliography of Fanon's writings, both political and medical, is appended.

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VINCENT M. BATTLE and CHARLES H. LYONS, editors. *Essays in the History of African Education*. (Center for Education in Africa, Institute of International Studies, Teachers College, Columbia University.) New York: Teachers College Press, Teachers College, Columbia University. 1970. Pp. ix, 123. \$3.50.

MICHAEL ANTHONY SAMUELS. *Education in Angola, 1878-1914: A History of Culture Transfer and Administration*. (Teachers College Studies in Education.) New York: Teachers College Press, Teachers College, Columbia University. 1970. Pp. xiii, 185. \$7.95.

Five theses and seminar papers submitted to the Center for Education in Africa, Teachers College, Columbia University, comprise the volume edited by Battle and Lyons. The authors and essays are as follows: Charles H. Lyons, "The Educable African: British Thought and Action, 1835-1865"; Henry John Drewal, "Methodist Education in Liberia, 1833-1856"; Vincent M. Battle, "The American Mission and Educational Development in the Southern Sudan, 1900-1929"; Priscilla Blake-more, "Assimilation and Association in French Educational Policy and Practice: Senegal, 1903-1939"; and Richard Heyman, "The Initial Years of the Jeanes School in Kenya, 1924-1931." These essayists have produced commendable studies, and their coverage of the sources is adequate, despite an overreliance in the first two papers on the admittedly excellent book by Philip D. Curtin, *The Image of Africa* (1964).

Several themes stressed by the essayists are familiar to the historian of Africa. They are the close relation between the development of educational and administrative policy, the impact of the missionary influence on education in sub-Saharan Africa, the parallelism in the civi-



lizing and evangelizing objectives of most missionaries, and the American influence upon African education.

In contrast to the diversity of topics in the essays, Samuels concentrates in his volume on the study of Angolan education in the 1878–1914 period. Consideration is given to the numerous forces that resulted in contemporary educational organization. The educational story also depicts the highlights of an important era in colonial history. Educational development is surveyed in accounts of the growth of governmental administration and Christian missions. Rivalry between Protestant and Catholic missions; the relative value of “civilizing” Africans through labor, language, and other cultural influences (*educação*) as opposed to traditional schooling (*instrução*); and the slow progress of the Portuguese and Angolan local governments in building an educational system are other themes. In both books the similarity in the interests of government and the missions, and, in the long run, the subordination of the latter to the former are obvious.

The Samuels study is based on an impressive array of sources as reflected in an exhaustive bibliography, and illustrative maps and an index are helpful. In modesty and understatement, the author displays a seasoned scholarship and masterful treatment of his topic. Samuels and the essayists emphasize the need for research on African education and suggest many topics awaiting the attention of historians. These volumes are recommended to specialists and those with a general interest in Africa.

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ALAN SCHAM. *Lyautey in Morocco: Protectorate Administration, 1912–1925*. Berkeley and Los Angeles: University of California Press. 1970. Pp. xi, 272. \$8.75.

Attention! The student of Moroccan affairs is likely to read the jacket copy, which does this book a disservice, and perhaps the first fifty pages, then lay this book aside as yet another rationalization of French colonialism (called *mission civilisatrice*, or “white man’s burden” in English). It would be a mistake. The introductory chapter relies on French sources, in-

cluding the writings of Lyautey himself, and repeats all the old clichés concerning roads, ports, dams, mines, and the necessity of treating the “natives” with understanding, respect, and generosity.

Even in the introductory pages, however, the author (almost unwillingly, it seems) exposes the paradox between Lyautey’s protestations and his actions, and in the final chapter provides a critical assessment of Lyautey’s failure to live up to his noble words. He points out that the French took over in Morocco to protect the Algerian border and French dominance in North Africa. The other ascribed reasons only supported that primary goal.

Lyautey, a royalist, was pledged by the Treaty of Fez to protect the authority of the sultan; he systematically destroyed it. He acknowledged the presence in Morocco of “cultured men who dealt as equals with European statesmen, who are skilled politicians and diplomats” (p. 29), but “owing to their lack of education in the modern European sense they were not regarded as capable of taking part in the French administrative mainstream” (p. 48). He never failed to insist that “one thing we have achieved: that is the knowledge of how to win the sympathy of the native. . . . Despite everything, these races have remained faithful to us,” but he did not live to see the conquest (he called it pacification) of these natives despite the tremendous military effort. Whatever may have been the excesses of his successors, Lyautey created the institutions and set the policy that led to the overthrow of the protectorate.

There is little new in the story of the French failure in Morocco. But the author has provided in *Lyautey in Morocco* something that is new: a detailed description of the governing institutions of the Sherifian Empire before and after the protectorate was established. Here lies the great value of this book. The author also has provided nine appendixes of reference material, a glossary of Arabic terms (many with meanings peculiar to Morocco), and an extensive and valuable bibliography.

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E. A. AYANDELE. *Holy Johnson: Pioneer of African Nationalism, 1836-1917*. (Africana Modern Library, Number 13.) New York: Humanities Press. 1970. Pp. 417. \$11.50.

Reverend James Johnson, a Sierra Leonean who became an assistant bishop in the Anglican diocese of Southern Nigeria, was tormented by a cultural schizophrenia reminiscent of Sun Yat-sen's on the other side of the world during the same early twentieth-century period. On the one hand Johnson was a man of his own people, passionately espousing African "purity," working for African self-government, and rejecting the materialism, the arrogance, and the whiskey-soaked immorality of Europe. On the other, he embraced European religion, including temporal forms and organization, with equal passion; he favored European science and technology and urged agricultural improvements and education in basic engineering for Africans; and he bitterly denounced much that was characteristic of African societies. With some of the British in Africa, on whom he looked with an increasingly complicated ambivalence, Johnson imagined that it would be possible to design and carry out a rational plan whereby Africa kept what was wholesome in her own traditions and adopted from Europe only those ideas and techniques that would harmonize with native ones. Inevitably he experienced disillusion and frustration. His sincerity, energy, and remarkable perseverance, however, brought him great fame throughout West Africa and in England. His accomplishments in proselytizing and in making Africans more aware of their own worth were more than enough to justify the subtitle of this book, even if the nationalism that finally emerged was as different from James Johnson's ideal as the kingdom of Italy was from Mazzini's.

Professor Ayandele of Ibadan has not produced an intimate portrait because, as he points out, he did not have access to personal papers. But this account, based mainly on prodigious research in the files of the Church Missionary Society, the Aborigines Protection Society, and the Public Record Office, is an authoritative and valuable rendering of the official side of Bishop Johnson's work. The author deserves high praise for his scholarly objectivity in an age when politics have intruded

into academic life rather more than one would wish.

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RICHARD WEST. *Back to Africa: A History of Sierra Leone and Liberia*. New York: Holt, Rinehart and Winston. 1971. Pp. 357. \$6.95.

Richard West, an English journalist, has attempted a comparative study of the "Westernized" settler communities of Sierra Leone and Liberia. (The book's subtitle inaccurately suggests a broader purview.) Parts one and two, together comprising nearly one-half of the volume, are interesting and well-written accounts of the colonization movements in Britain and the United States and of the founding of the settlements at Freetown and Monrovia. West takes a largely biographical approach, with numerous quotations from the writings of Granville Sharp, Zachary Macaulay, Jehudi Ashmun, R. R. Gurley, and other participants. He has a flair for selecting apposite quotations, and he explores a number of topics passed over or given bare mention by other writers. Parts three and four survey the histories of the settlements during the last two-thirds of the nineteenth century and the early years of the twentieth, with a chapter on Marcus Garvey and his Back To Africa movement. West incorporates copious extracts from foreign observers, including Richard Burton (inevitably), Harry Johnston, and Mary Kingsley, from Edward Wilmot Blyden, and from James Africanus Horton, Benjamin Anderson, and a few other Creoles and Americo-Liberians. The selections on Sierra Leone include choice samples of European condescension and defamation balanced to some extent by critiques of the authors and some colorful passages on the drinking habits and other excesses of resident Europeans. West's presentation has serious limitations: quote—commentary—quote is no substitute for systematic historical reconstruction and analysis. Part five is a brief fifty-page account of developments in the past half century, concluding with some surprisingly shallow and trivial reminiscences from the author's two-month trip to Sierra Leone and Liberia. The reader has cause to wonder what perspectives

West, the journalist, gained from doing historical research.

In sum *Back to Africa* is a very uneven piece of scholarship. West fails to provide more than superficial descriptions and analyses of Creole and Americo-Liberian societies. Many topics are poorly researched, and there are numerous errors of fact or interpretation that could easily have been avoided. The method of footnoting is haphazard: the reader wants to know where all the quotations were found and to whom the author is indebted for opinions and interpretations. Nonetheless some sections are very good, with excellent use of quoted material coupled to stimulating commentary. The illustrations are well chosen to complement the narrative. If the specialist will be disappointed—or annoyed—by West's treatment of some topics, it is only fair to acknowledge that the general reader will find *Back to Africa* a highly readable and frequently entertaining introduction to Sierra Leone and Liberia.

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S. A. AKINTOYE. *Revolution and Power Politics in Yorubaland 1840-1893: Ibadan Expansion and the Rise of Ekitiparapo*. (Ibadan History Series.) [New York:] Humanities Press. 1971. Pp. xxi, 278. \$10.00.

This is an important study, adjusting as it does the perspective on nineteenth-century history of the key Nigerian group known collectively as the Yoruba. For the marginal effects of European colonialism in Africa have included not just the Euro-centered view of history, now finally vanquished, but the more subtle tendency in restudying and revising to deal with those Africans with whom Europeans were first preoccupied. Thus in Nigeria students of African history have looked either at the coast and its immediate hinterland, or at the Islamic empire of the Fulani and its predecessors in the north.

Akintoye takes us further inland than have previous studies in Yoruba history (the notable exception is Johnson's *History of the Yorubas*, written in the 1880s and as much a source as a historical account). Himself Ekiti (that is, from one of the Yoruba subgroups central to his study), Akintoye has that invaluable insider's ability to obtain and utilize oral evidence. Nor has he neglected written material, including

British sources. He also handles with detachment the role of Ibadan, that most formidable Yoruba power and opponent of the Ekiti and other subgroups of northwestern Yorubaland; he presents, indeed, the most valuable analysis of nineteenth-century Ibadan's impact available in print to date.

As the title and subtitle indicate this is a study in military and diplomatic history. It is mainly a book for specialists; the novice in African history may lose his way amid the many names of people and places and positions (one could wish for more and clearer maps, particularly for one that put Yorubaland in Nigeria).

Nonetheless, the book is of more general interest. It gives an excellent and concise introduction to the whole of eighteenth- and nineteenth-century Yorubaland. It treats interestingly the question of Yoruba unity or lack thereof, a question that continues to matter in twentieth-century Nigerian politics. Further, historians with interests elsewhere on the continent and beyond will find valuable material for comparative purposes. For, always with extensive evidence carefully examined, Akintoye tackles such problems as how urbanization took place; how a state whose expansion was military incorporated refugees and the conquered; and how related African groups, threatened by signs of European advance, did or (as in this case) did not perceive the threat—or did not regard it as of sufficient importance to set aside differences. Here, too, are examples of an African provincial, or even perhaps colonial, administrative system; of a confederal organization formed for military purposes; and of the continuing role of "domestic slavery" in politics, economics, and the army.

A synthesizing conclusion as illuminating as the introduction would have been welcome. Still, each of the author's analyses emerges with clarity throughout the book, making it a valuable and stimulating contribution.

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State University College,  
Purchase

HARRY A. GAILEY. *The Road to Aba: A Study of British Administrative Policy in Eastern Nigeria*. New York: New York University Press. 1970. Pp. x, 184. \$7.50.

Professor Gailey is concerned with four linked

topics: the establishment of British rule in southeast Nigeria; the imposition of an apparatus of local government inspired by Lugard's Northern Nigerian system; its manifest unpopularity, culminating in the women's riots; and, tentatively and briefly, Sir Donald Cameron's reforms. The book is briskly written and clearly organized, no slight achievements given the multiplicity of groups involved.

Gailey confesses that conditions in Eastern Nigeria in the spring of 1967 led him to abandon the idea of interviewing the "old men and women in the villages" in order to analyze the riots. Consequently he has written an essentially administrative history, with the rioters seen mainly through administrators' eyes. Yet it is administrative history of a remarkably rich kind. Lugard, after dismissing the anthropologist employed by the Southern Nigerian government, succeeded in making his own views prevail, but there was no monolithic unity of opinion within the bureaucracy. Disagreement was sharp and articulate, allowing Gailey to reveal the issues at stake in the dialectic of policy making.

He shows how alien district officers, parvenu warrant chiefs, usurious clerks and court messengers usurped the traditional authority of the village elders. When the riots revealed the unpopularity of this regime to all but the most complacent administrators, Cameron set about establishing institutions of local government more in accord with the precolonial pattern.

Gailey gives tantalizing glimpses of the social history he was forced to abandon. The Dancing Women's Movement, the Spirit Movement—which attacked non-Christians, especially chiefs and their families—and the remarkable leadership and discipline of the Oloko women, all deserve extended examination while some of the "old men and women in the villages" still survive. When the social history of protest in southeast Nigeria is studied in depth and compared with similar movements elsewhere in Africa—and in other continents—the significance of the Aba riots, and the British bureaucratic mistakes precipitating them, will be better judged. Meanwhile, Gailey has written an extremely useful case study of British colonial officialdom in crisis.

HENRY S. WILSON  
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J. S. LA FONTAINE. *City Politics: A Study of Léopoldville, 1962–63*. (African Studies Series, Number 1.) New York: Cambridge University Press. 1970. Pp. xiii, 246. \$13.50.

Few cities have undergone such sudden and profound change as Leopoldville at the time of La Fontaine's study in 1962–63. In the three preceding years, the city had not only become the capital of an independent Congo but had doubled in population, since it was a rare island of security in a country torn by civil war. To understand the effects of these phenomena the author conducted her own surveys of two of the city's communes, which she then integrated with other available political and social studies creating a general synthesis.

The strength of La Fontaine's book resides in its examination of the network of personal contacts between ordinary Zairois and their leaders. Discussing the fundamental problem of housing, she sensitively analyzes the relationship between tenants and landlords. In the realm of politics, she specifies the qualities necessary for success in both national and local office, attributes she calls publicity and patronage.

La Fontaine's broader interpretation, however, is gravely contaminated by her reliance on the distinction, so dear to colonial social anthropologists, between traditional and urban Africans. According to this view migrants to the city arrived conditioned by the static organization and kinship network of their villages, and they only gradually learned to subordinate these "tribal" characteristics to more modern political behavior. The argument carries with it the implicit assumption that rural Africans remained virtually unaffected by colonial rule until they moved to a city.

This viewpoint seriously distorts the colonial reality. Africans migrated to the cities after their old way of life had been substantially modified by the demands of colonial governments. Thus, colonial urbanization was not so much "a means of repudiating the traditional" (p. 149) as the most important in a continuous series of changes that transformed African society as it had existed before the European conquest.

BRUCE FETTER  
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DOUGLAS L. WHEELER and RENÉ PÉLISSIER. *Angola*. (Praeger Library of African Affairs.) New York: Praeger Publishers, 1971. Pp. ix, 296. \$11.00.

Though several works on various aspects of Angolan history have appeared in recent years, *Angola* is perhaps the first attempt at a general synthesis within a single volume. It is possible that such an attempt is premature. As Wheeler himself observes, "The pre-European history of Angola is one of the least known histories of tropical Africa." Monographic studies of the early colonial era (1575-1800) have thus far raised as many questions as they have answered and do not lend themselves to a brief and uncontroversial summing-up. Consequently specialists in central African or Portuguese colonial history may find *Angola* much less satisfying than will the nonspecialist public for which it appears to have been written.

The abortive 1961 rising against Portuguese rule is clearly the focal point of the book. Approximately a quarter of the text is devoted to the rising and its aftermath. This is Pélissier's contribution. It is a painstaking and dispassionate examination of the evolution of the major nationalist factions, their respective achievements and failures, and the Portuguese response to the kinds of threats and opportunities that a divided nationalist movement offered. Pélissier's assessment of the contemporary situation is that the nationalist guerrilla campaigns have, ironically, provoked "an unprecedented stimulus" to Angola's economic development since 1960. He believes that the future of Angola will depend more upon the generation of Africans now enrolled in the greatly expanded secondary school system than upon the nationalist activists presently engaged in the liberation movement.

Wheeler's main contribution to the volume takes the form of an extended prelude to the section written by Pélissier. This format has the advantage of providing thematic continuity, though it runs the risk of seeing the Angolan past largely in terms of a sort of evolutionary movement toward the 1961 rising. Thus he repeatedly alludes to massive Bakongo popular resistance to Portuguese influence ("the African jacquerie") beginning as early as the sixteenth century. As presented, Wheeler's case for widespread protonationalist "jacqueries" rests more

upon assertion than upon evidence. On the other hand his treatment of the emergence of *assimilado* protest during the nineteenth century is well documented and is appropriate to an interpretation that focuses on the growth of African political consciousness and on the rising of 1961.

As a general survey of those aspects of the Angolan past that help illuminate the crisis of 1961 and as an analysis of events between 1961 and 1971, *Angola* is genuinely useful. And it is in this period—the almost contemporary past—that the bulk of its prospective readers are most likely to be interested.

F. JAMES BERG

Colgate University

ROBERT HEUSSLER. *British Tanganyika: An Essay and Documents on District Administration*. Durham: Duke University Press, 1971. Pp. xii, 154. \$6.75.

The colonial district officer deserves the attention of historians, be they students of modern Africa or of the imperial process. Local administrators enjoyed their "heroic" era, in general, before the First World War: that is, before the crystalization of central bureaucratic structures, a time when initiatives were idiosyncratic and responsive to local political currents. Robert Heussler is not mistaken, therefore, in choosing to focus upon district officers; but he has failed to understand the contexts, regional and temporal, that defined the scope of the individual's activity.

By turning a blind eye to the German period, the formative colonial years in Tanganyika and surely the heroic time of district administration, Heussler makes an inauspicious start. He then takes up the various aspects of an official's task, with an extraordinary disregard for periodization and chronological sequence that will frustrate those who look for clues to the development of administration in the country. The essay illustrates a methodological trap well known to historians and others dealing with oral tradition, for it is only an enlargement of the "actors' model" of events. District officers often felt isolated at their posts and out of sympathy with dictates from the capital, preferring to see themselves as local barons. To be persuaded that these men did in



fact have great influence, we must be told far more about local politics and the historical circumstances of the respective districts.

The occasional merits of this short essay and the accompanying illustrative documents hardly compensate for the stereotypes of African society and politics it retails. A thoroughly researched study of the district officer will, among other things, draw on African evaluations.

MARCIA WRIGHT  
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GERALD L. CAPLAN. *The Elites of Barotseland, 1878-1969: A Political History of Zambia's Western Province*. Berkeley and Los Angeles: University of California Press. 1970. Pp. xi, 270. \$8.50.

This is the history of the Lozi, the dominant grouping of Barotseland, centered on the flood plain of the upper Zambezi in present-day Zambia. Its ruling class traditionally held ties with the white establishment in Southern Africa; because of this and by virtue of their strategic location, the Lozi have constituted a political nexus of far greater importance than numbers alone would have warranted.

One should not take this book as yet another parochial study by an expatriate. This is a significant achievement, of merit equal to Ajaye's *Christian Missions in Nigeria* (1965), or Ogot's *History of the Southern Luo* (1967), in both methodology and synthesis. Although it breaks some new ground, its real benefit remains as an analytical and interpretive history, balancing both previously explored and new sources, affording insight into any number of historical issues claiming universality far beyond Barotseland's borders. Here is played out the vain attempt of a great African kingdom to prevail by accommodating rather than resisting European power, battling (but never stemming) the relentless erosion of its powers by the "indirect rule" that ensued. We see the pivotal role of "Western" (mission) education in the formation of new elite groups, whose destiny the missions—and governments—ultimately failed to control. We are shown how structural underdevelopment in a colony, customarily interpreted as imperial absent-mindedness, is often a conscious policy to further imperial aims and how

"tribal clashes" are class conflicts by another name. Finally, we see the sorry consequences of a traditional ruling class allying with European interests to suppress the rise of the new elite and in the process secessionist movements being born.

The book essentially depicts the interaction between European power (mission, chartered company, and colonial administration) and two African elite groups: initially the Lozi establishment, and subsequently the mission-educated elite. The latter, which was to become the backbone of the United National Independence party, ultimately tested the Lozi traditionalists, and after the Europeans departed, prevailed. Professor Caplan masters a variety of sources, oral and written, to chronicle these highly complex themes with a sure touch. He details King Lewanika's symbiotic relationship with the missionary Coillard, leading to the signing of the fateful (and fraudulent) Lochner Concession. The king's subsequent realization of what he had done and his efforts to turn aside the consequences contain all the elements of a Greek tragedy. Lewanika's aims of fashioning an educated elite to modernize and protect his lands were dashed as the British South Africa Company reneged on its commitments, arrogated his powers unto itself, and forced him into a final, groveling capitulation. In the process his "modernizing" elite developed such a stake in preserving itself and the monarchy that it became entrenched as the royalist party, utterly reactionary and manipulated by the Europeans, until its downfall on their departure.

Caplan might well have detailed more thoroughly the educational process by the missionaries, which produced the new middle class, and he might have exposed the questionable relationship between missionary and company. Some of the main figures, again particularly the missionaries, remain shadowy. But no matter: this is an important book, carefully constructed, painstakingly researched, and packed with detail. For any graduate student embarking on African field research, Caplan's discursive passage on the nature of his evidence ought to be required reading.

ALAN R. BOOTH  
Ohio University

LEONARD THOMPSON, editor. *African Societies in Southern Africa: Historical Studies*. (Published under the auspices of the African Studies Center, University of California, Los Angeles.) New York: Praeger Publishers. 1969. Pp. vii, 336. \$8.75.

Since World War II African history has become a "growth discipline," and its methods and perspectives are now being applied in Southern African studies. Leonard Thompson, who wrote a major work well within the traditional mode of South African history, has been eager as teacher and scholar to adopt the new approaches. In 1968 he organized a conference at the University of Zambia; this volume contains thirteen papers from that conference, six of them by former students. The papers are: Leonard Thompson, "The forgotten factor in southern African history"; D. W. Phillipson, "Early iron-using peoples of southern Africa"; Brian Fagan, "The later Iron Age . . ."; Monica Wilson, "Changes in social structure . . . : the relevance of kinship studies to the historian"; Martin Legassick, "The Sotho-Tswana Peoples before 1800"; Shula Marks, "The traditions of the Natal 'Nguni': . . . the work of A. T. Bryant"; Gerrit Harinck, "Interaction between Xhosa and Khoi . . . 1620-1750"; Alan Smith, "The trade of Delagoa Bay as a factor in Nguni politics"; William Lye, ". . . Sotho peoples after the Difaqane"; John Omer-Cooper, "Aspects of political change in the Mfecane"; David Hammond-Tookes, ". . . a model of Cape Nguni political process"; Richard Brown, "The External relations of the Ndebele kingdom . . ."; Anthony Atmore, "The passing of Sotho independence 1865-70"; and Colin Webb, "Great Britain and the Zulu people 1879-87." Two of the authors are archaeologists, two anthropologists, and ten historians. Although the papers vary in readability, the standard is uniformly high.

Volumes of papers whose only common interest is in the past of a particular region inevitably invite the criticism that the papers could have been dispersed as articles in journals. The point is well taken, but it can be partially met by arguing that this is a book for Southern African specialists. More seriously, these papers, on the whole, lack succinct endings to make clear the authors' precise conclusions and their significance for a historian, though the reader

is given real help by the editor's valiant introduction. Second, in describing migrations that lack a unifying thread of some sort and that involve a large number of names in addition to those of places, clear exposition in relation to adequate maps is essential. The maps included are excellent, but they are placed at the end of each paper, and the narrative and analysis are not explicitly connected with them.

These papers raise general issues; there is space to consider only two of them. Political pressures, to which all Southern African scholars are subject, give to the phrase "from an African point of view" a treacherous ambiguity. The detachment of the authors of these papers is impressive though it is due in part to the fact that the subjects avoid political issues. However, Atmore's conclusion, in which the end of Sotho independence is seen in terms of "white control," "interwhite conflicts," and "white interference" (pp. 300-01), overlooks the fact that it was hardly unimportant—either historically or to the Sotho—whether the Orange Free State, Natal, Cape Colony, or the High Commissioner were responsible for the governance of Lesotho. It would be a pity if the older South African history, which ignored variety among Africans, were to be succeeded by one that lumps all whites together.

Second, there is the question of the role of narrative and its relation to interpretation. Historians are not concerned only with event, but also with process and event. But what is the value to the historian of the study of the migration and subsequent distribution of peoples or the impact of languages on each other? What story is the Southern African historian trying to tell? Are these studies of languages, migrations, and consequent distributions to become the very stuff of history, or are they aids to interpretation?

The balance between narrative and interpretation can be seen in two of the papers dealing with the Mfecane. Smith's paper on trade is ingenious and careful, but it involves large assumptions; for example, ". . . it is improbable that this caravan could have originated in any place other than Zululand" (p. 187). Furthermore, so many statements have to be qualified that the paper becomes suggestive rather than conclusive. Unfortunately, the paper ends abruptly, and the material is not exploited.

Omer-Cooper, working on the basis of his earlier pioneer essay, *The Zulu Aftermath* (1969), is frankly interpretive, but rather than qualifying his statements, he appears to adopt a major thesis on continuity in the Zulu kingdom on the basis of an assertion that certain "innovations" were not "radical" (pp. 216, 227). Although sorting out events and interpreting them must go on together, do we yet know enough about the Mfecane itself—about the sheer flux of events—to justify interpretations for which it may be difficult to find evidence?

Professor Thompson and his colleagues raise an important set of issues. Many Southern African historians have turned away from the traditional questions because the approach was too political and too preoccupied with whites. But what central theme is to replace the old one of the development of the state and the impact of its policies? In other words, are the new historians concentrating sufficiently on problems of narrative, dealing with events in which Africans were the sole participants, or others in which only the "white side" has been subjected to detailed examination?

JEFFREY BUTLER  
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R. RAVEN-HART, translated with notes by. *Cape Good Hope, 1652-1702: The First Fifty Years of Dutch Colonisation as Seen by Callers*. In two volumes. Cape Town: A. A. Balkema. 1971. Pp. xv, 222, xi, 223-527. R. 30.00 the set.

This is a paste-and-scissors work, but excellent of its kind. The subject is a pretty well-worn one, as books by visitors to the Cape in the colonial period form a favorite quarry for South African historians. For example, the travel literature relating to the Cape has been extensively used by the editors of the Van Riebeeck Society's publications and in works like Edward Strangman's *Early French Callers at the Cape* (1936). However, Raven-Hart has exhumed some unpublished accounts from the archives at Cape Town, The Hague, and London, which he has dovetailed with the better known published ones.

The arrangement is strictly chronological, thus involving the separation into two parts of accounts that describe the Cape on the writer's homeward and outward voyages. Each account is accompanied by a briefing (if I may use the

term) on the historical background relating to the individual's visit. A very full index, combined with a bibliography and glossary, facilitates consultation of the work, once the reader understands its rather complicated arrangement. The accounts themselves vary greatly in extent and in interest. Some of them, like those on pages 412-16 and 447-51, are such small beer as to be hardly worth printing. The hitherto unpublished accounts do not add anything material to what we already know; but the editor-translator judiciously evaluates them all and indicates where they confirm or contradict each other. The great majority of the callers of all nationalities were far more interested in the Hottentots than in the white inhabitants of the Cape. This interest is reflected in the index, which has thirteen and a half columns of references to the Hottentots and their activities.

Judging from the preface this work is perhaps chiefly intended for persons living in South Africa who have only limited knowledge of seventeenth-century history. The two volumes are richly illustrated; but unfortunately the reproductions have been made from photographs of the original engravings and illustrations, thus making many of them rather smudgy and inferior. Raven-Hart's enthusiasm for his self-imposed task is infectious; but although this work may well make fascinating reading for *aficionados* of early South African history, it is dispensable for anyone else.

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RAYMOND K. KENT. *Early Kingdoms in Madagascar, 1500-1700*. New York: Holt, Rinehart and Winston. 1970. Pp. xvi, 336.

In this seminal study Kent accomplishes a major reinterpretation of Malagasy history. Previous interpretation, which generally rejected the African contribution to the state-building process in Madagascar occurring after 1500, had been conditioned by the massive scholarship of Alfred Grandidier and his son Guillaume. Nevertheless, there were serious flaws in the work of the Grandidiers and their successors, which Kent demonstrates by a careful analysis of the written sources they used, and by an equally searching analysis of an im-

pressively wide range of oral, linguistic, and other nonarchival evidence.

Kent's conclusion is that early in the first Christian millennium migrations from the Indonesian archipelago brought many varying groups of these islanders to the western Indian Ocean world where they spread widely into east central and southeast central Africa, this migration taking place before the arrival of the Bantu-speaking peoples into these regions. As the Bantu migrations continued, an Afro-Malagasy "race" resulted, of varying cultural composition due to the process of contact in each locality. This pressure also led to Indonesian settlement in Madagascar, where previously only a limited number had gone, thus establishing the lasting Indonesian language base for the peoples of the island. The process was largely completed by the tenth century. Then early in our second millennium, when Indonesians on the African continent were confined to regions on the eastern coast and in the south, new pressures from Bantu-speakers and from the east-coast Muslim trading communities caused the last important movement of Indonesians to Madagascar. The migrants had by this time absorbed much from the Bantu cultural milieu, and some spoke only Bantu languages. Their movement ended by the end of the fourteenth century, and they set the stage for future developments on Africa's greatest island.

In this painstaking study, with its extensive references demonstrating the widest use of all forms of evidence relating to Madagascar's past, Kent has made a lasting contribution to the island's history. His conclusion is now established: "In short, the traditional past of Madagascar makes absolutely no sense without Africa" (p. 265).

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#### ASIA AND THE EAST

PAMELA NIGHTINGALE. *Trade and Empire in Western India, 1784-1806*. (Cambridge South Asian Studies, Number 9.) New York: Cambridge University Press. 1970. Pp. xv, 264. \$11.50.

OLE FELDBÆK. *India Trade under the Danish Flag, 1772-1808: European Enterprise and Anglo-Indian Remittance and Trade*. (Scandi-

navian Institute of Asian Studies Monograph Series, Number 2.) [Lund:] Studentlitteratur. 1969. Pp. 239. 45.75 S. kr.

The years from 1757 to 1818 can be characterized as the period of consolidation of English power in India. Throughout this period British policies were molded by the internal situation in India, the actions of other European powers, and the commercial interests of both the East India Company and the servants of the company who traded privately. The role played by British commercial interests in the extension of British control in western India and in the activities of the Danes is examined by Pamela Nightingale and Ole Feldbæk respectively.

Dr. Nightingale's thesis is that British imperialism in western India was not imperialism primarily for political gain and that the economic considerations of both the company and the private traders led to the consolidation of British authority over Malabar and Gujarat. With clarity and style Dr. Nightingale discusses her findings and in so doing not only has accomplished the affirmation of her thesis but has differentiated two types of economic imperialism: imperialism for the creation of a commercial monopoly and imperialism for the creation of stable conditions that allow for the pursuance of commercial goals. The transition between these two commercial philosophies by the English East India Company in western India has been accurately depicted in the examination of British policy toward Malabar and Gujarat.

Mr. Feldbæk in his examination of "the state of things behind the Danish facade" (p. 7) has shown the effect of British policies on the fortunes of the Danish Asiatic Company and private Danish traders. His findings regarding the official commercial activities of the Asiatic Company are not new and in essence reaffirm the notion that as British power increased, the commercial prospects of the Asiatic Company declined. What is unique, however, is the detailed account of the private Anglo-Indian commercial activity carried on under the Danish flag. This is a story that has not been told before with such thoroughness. From this account an appreciation can be had of the magnitude of these Anglo-Indian remittances as

well as the important role that the Danes played in the continuance of clandestine English private trade.

The advantage that Dr. Nightingale had, which Mr. Feldbæk did not, was the availability of private correspondence and commercial documents through which she was able to reconstruct the function of both the collective interests of the private commercial community and individual relationships in the formulation of British policy in western India. Mr. Feldbæk, on the other hand, had to glean his information regarding private trade chiefly from English, French, and Dutch sources, as few of the private Danish papers are extant. Whatever Mr. Feldbæk's account lacks in intimacy is compensated for by the volume of data cited and the number of examples given.

The only noticeable deficiency in Dr. Nightingale's work is the absence of a glossary that would have removed any possible vagueness regarding the meanings of such terms as *adowlat* and *chauth*, which are used but not defined in the text. The principal weakness in Mr. Feldbæk's study is likewise not one of substance but one of form. Within the body of the text he has included so much data that at times the flow of the discussion is disturbed. Some of this data should have been added to the excellent appendixes, which include valuable information on currency equivalents, values of cargoes, and the profitableness of Indian cargoes.

Within their defined areas of inquiry, these two works should be considered essential secondary sources for those interested in understanding the motives, means, and consequences of the consolidation of British power in India.

ANN BOS RADWAN

*University of Pennsylvania*

H. I. LONDON. *Non-White Immigration and the "White Australia" Policy*. New York: New York University Press. 1970. Pp. xv, 318. \$6.95.

EGON F. KUNZ. *Blood and Gold: Hungarians in Australia*. Melbourne: F. W. Cheshire. 1969. Pp. xviii, 301. \$7.50.

ALBERT A. HAYDEN. *New South Wales Immigration Policy, 1856-1900*. (Transactions of the American Philosophical Society, New Series, Volume 61, Part 5.) Philadelphia: the Society. 1971. Pp. 60. \$2.25.

Although these books are concerned with Aus-

tralian immigration, their particular topics necessitate individual appraisal. Dr. London's book describes how a population-hungry Australia rationalizes the exclusion of most non-white people. He reports that in 1968 there were 41,000 non-Europeans in Australia, of which group only 20,000 were Australian citizens. He feels that Australia may have to terminate its policy of "Australianizing" all immigrants and to accept some newcomers who may not be socially assimilable. Most Australians, he admits, prefer their homogeneous society—even long-resident Australian-Chinese seem to prefer "White Australia" to a nation open to major influx from Asia.

The author notes that the policy has been moderated to a degree during the sixties. Reform groups have developed. Politicians have grown sensitive to the criticism of excolonial nations in the UN. The increased number of Asian students at Australian universities has triggered student activism against the White-Australia policy. Polls and the press show some change in public attitudes—wide sympathy, for example, toward the Asian victims of such *causes célèbres* as the Nancy Prasad case. There is a feeling that Australian population needs can no longer be provided wholly from Europe and that the present policy of exclusion may imperil national security by isolating Australia from her Asian neighbors.

The Australian rationale against admitting large numbers of non-Europeans emphasizes the point that unassimilated pockets of immigrants endanger social harmony, might lead to racial violence, and might even embroil Australia with Asian nations. There remains some belief that nonwhites are, in certain respects, inferior to white Europeans. There is a decided sentiment that Australians have the right to decide who shall dwell among them. The major political groups tend to reflect this view although the Roman Catholic Democratic Labor party, the Communist party, and some Christian churches condemn the exclusion policy. The Liberal-Country administration also carried out some modifications in 1966, which have increased immigration opportunities for nonwhites, though still favoring the degree-holding, Westernized elite of this category. The Australian Labor party's traditional opposition



to "cheap colored labor" continues but may be liberalized under the new leader, E. G. Whitlam. All parties have taken note of the racial problem in the United States and in Britain and of the racialism and exclusivist legislation by Asian and African states.

The author, who believes a more liberal policy and, ultimately, an end to racially defined exclusion must come, has presented a readable, interesting account. He seems, at times, a bit unfair in his criticism of Australians for occasional boorishness toward nonwhites or neglect of social contact with such immigrants. Australian standards are no worse than those of any other nation with an immigration problem. And relations between Australians and white "New Australians" suggest that Australia may not be ready for more complex large-scale immigration problems.

Dr. Egon F. Kunz presents a somewhat romantic view of Hungarian immigration to Australia, a very small element of its human influx. We see the ebb and flow of a basically reluctant immigrant group, pulled toward Australia by gold, liberty, and sanctuary or pushed by poverty, war, injustice, and rebellion. The waves began with humble, largely Jewish arrivals in the 1830s, to be followed by the larger Magyar groups of refugees from the 1848 revolution, by gold seekers, and by refugees from the impact of the two World Wars, from the advent of communism, and finally from the invasion of Hungary by Soviet Russian in 1956.

The Hungarians seem always to hope for a return to the Central European homeland and so remain quietly Magyar in life-style and rather neutral in politics, although very active, Dr. Kunz reports, in all other fields of endeavor, especially the arts, sciences, and university teaching. Hungarian Protestants have moved into the Anglican fold in Australia, but Catholics have adjusted only with difficulty to the peculiarities of the Irish-Australian Catholic community. The rise of "Little Budapests" should be balanced against the record of considerable intermarriage with Australians of British stock. The author's style is at times repetitious, and there are too many lists in the text that really belong among the footnotes. There are occasional puzzling statements such as a reference to an 1854 voyage of the ship

*Golden Age*, "the first ship ever to go from Australia via the Panama Canal to England" (p. 69).

In contrast to Dr. Kunz's book, Professor Hayden's short study is excessively compressed. It takes up the Australian immigration question for the period 1856-1900, following approximately on the heels of R. B. Madgwick's book covering 1788-1851 and treating only New South Wales. Detailed accounts of successive governments of rough-edged politicians are presented, showing where the continually improvised policies for assisted or nonassisted immigration were influenced by the pastoralists' fear of recruiting more free-selectors, and by labor's suspicion of immigrant competition, and by the fluctuations of economic conditions limited by the rather earth-bound decisions of local political warfare. The author believes that while economic forces played a large part in shaping immigration legislation, the politics of land and of the wider franchise created political priorities that sometimes overrode or smothered economic requirements.

CHARLES S. BLACKTON  
Colgate University

#### AMERICAS

CARL ORTWIN SAUER. *Sixteenth Century North America: The Land and the People as Seen by the Europeans*. Berkeley and Los Angeles: University of California Press. 1971. Pp. xii, 319. \$10.95.

Sauer uses sixteenth-century accounts of the North American continent to reconstruct the conditions before Europeans came to stay, to indicate the incipient change induced by their contact, and to obtain "an overview of what this country was." This is not, however, a source book. The geographer dominates the page, narrating the context of experience in which the observations cited were made. For example, Hernando de Soto is properly introduced as a brutal predator, the sources for his expedition are briefly described, and its organization and its progress rehearsed. We follow the march accompanied by a guide who continually cites contemporary report with a running commentary approving, questioning, or correcting it. The method is to observe observation;

the style is dry, clipped, and packed with fact; the tone is neutral. Thus, Sauer has attempted here for North America what he achieved so brilliantly for the Caribbean in *The Early Spanish Main* (1966). Once again we find the grasp of physical geography and occasionally the insight into the mental process of discovery that marked the earlier book, though here the material, one feels, is less familiar to the author and the subject less confined in area and period. As one might reasonably expect, the sections on Spanish exploration of the south are the best—happily indeed for they make up the greater part of the book. European preconceptions and attitudes, which of course shaped the explorers' impressions, should perhaps receive more attention in a round and fair view of the historical event, but Sauer's mind is quite legitimately on the object rather than the subject and on the geographical rather than the anthropological aspects of the object. He shows that object—the land with its endowment of vegetable, animal, and human life—to have been in many ways more admirable than latter-day Europeans or Americans have imagined and as yet little affected by European intrusion. We may with some confidence hope that Sauer's close adherence to the narrative records and his cool realism will prove as salutary to students as they are refreshing to a historian.

K. R. ANDREWS  
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C. HARVEY GARDINER. *William Hickling Prescott: A Biography*. Introduction by ALLAN NEVINS. Austin: University of Texas Press. 1969. Pp. xxi, 366. \$7.50.

JOHN HEMMING. *The Conquest of the Incas*. New York: Harcourt Brace Jovanovich. 1970. Pp. 641. \$12.50.

Many years will doubtless pass before another biography of Prescott appears, so thoroughly has Professor Gardiner researched the life and times of this Boston brahmin who produced classical works on the history of Spain and its conquests in America. Certainly it will be difficult to turn up much new meaningful material on his family, education at Harvard, marriage, friends, search for a suitable topic to write on, and the life-long vigil to conserve his eyesight.

As one reads the mass of minutiae presented to document the progress of Prescott from his birth in 1796 into a prominent family until his death in 1859, "attended by several doctors," one wonders whether any historian in the United States has provided for posterity as much information on himself and his historical methods as did Prescott—with the exception perhaps of H. H. Bancroft's *Literary Industries* (1890). We see the well-to-do clubman struggling with the competing claims of social life, family responsibilities, and persistent illnesses; his constant exhortations to himself to conserve his time and energy for serious studies; his preoccupation with the way his books were published—the paper used, illustrations, color of bindings, copyright problems, royalty arrangements, and dispatch of review copies to promote sales.

We are, in fact, told too much. Professor Gardiner might well have heeded Prescott's own challenge to himself: "Do not be afraid of being too brief." The reader wearies of the almost daily health reports and the chitchat about social trivia and his children, though we learn almost nothing about his wife.

The most significant omission is any detailed or analytical evaluation of Prescott the historian. From the scholar who has made himself the foremost authority on the subject, we might well expect a more substantial treatment. Why is Prescott's *Ferdinand and Isabella* (1838) "distinctly Prescott's masterwork" (p. 145)? Is it true that "scholarly myopia attends most judgments of Prescott's histories" (p. 143)? Why did the American Historical Association in 1949 select another wealthy and socially prominent Bostonian, Francis Parkman, along with Frederick Jackson Turner, when the Pan American Institute of Geography and History requested the names of representatives and significant American historians? Was this myopia, a mature judgment, or ignorance of the true merits of Prescott's *Conquest of Mexico* (1843) and *Conquest of Peru* (1847)? Despite the labor lavished during the last decade and more by Professor Gardiner on Prescott, he gives no answer to these fundamental questions.

*The Conquest of the Incas* demonstrates once more that the writing of history cannot be left exclusively to the professional historian. It

also reflects how much has been learned about Inca history and Spanish action in America since Prescott bravely tackled the same subject over a century ago. Mr. Hemming has profited from the many contributions of widely scattered specialists, but it is the combination of his own imagination, diligence, and attractive style that has enabled him to produce the first satisfying and sound general work on the conquest of Peru that has appeared in a generation. Even this gifted writer, however, has been unable to escape the fatal fascination of the tangled genealogical story of Inca descendants as they struggled to establish their claims with the Spanish bureaucracy. And though he has missed a few useful items by Marcel Bataillon and Alfonso García Gallo, the only serious omission appears to be the works of the Swedish ethnohistorian Åke Wedin. (See Magnus Mörner and Hans Andersson, "A Reappraisal of the Sources of Inca History: The Works of Åke Wedin," *The Americas*, 25 [1968]: 174-90.)

What makes this comprehensive treatment so valuable is the author's fresh look at the conquest, from which has resulted a balanced view of this complicated process, including Inca reactions and viewpoints on Spanish rule. The narrative flows along smoothly, and there is due attention to Spanish legal and religious preoccupations. Excellent notes, maps, and illustrations plus a good index add to the value of the volume.

Mr. Hemming closes his work with an account of the modern explorers, especially Hiram Bingham, who sought for the lost city of Vilcabamba. As every true historian of Peru wants to do, Mr. Hemming made his own explorations and had adventures too. He visited one of the Inca sites at Choquequirau after a perilous trip across the Apurímac canyon by means of strands of telegraph wires: "I tied myself to a curved piece of wood that slid along these wires, with my feet dangling above the swirling grey waters, and hauled across with my arms for 250 feet along the swaying wires" (p. 481), a fitting finale for a rich experience in research and writing.

LEWIS HANKE  
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EUGENIA MEYER. *Conciencia histórica norteamericana sobre la Revolución de 1910*. (Series Historia, Number 22.). México, D. F.: Instituto Nacional de Antropología e Historia. 1970. Pp. 234. \$3.60.

North American observers of the Mexican Revolution of 1910 have over several decades tortuously wrenched themselves from an inherited negativism toward everything Mexican and launched themselves into a commendable but perhaps futile search for the *realidad mexicana*. So argues Eugenia Meyer, a Mexican historian, in her analysis of changing American attitudes toward the revolution in this important, though incomplete, historiographical essay.

Trapped by their Protestant Anglo-Saxon prejudices concerning racial mixture, the Roman Catholic Church, and the Spanish tradition, most American authors writing from 1910 to the 1930s considered the revolution a logical and to-be-expected extension of Mexico's chaotic past. The rebels, they contended, heedlessly dismantled the peaceful and progressive state established by Porfirio Díaz and justified later United States intervention by proving their inability to govern themselves.

Events surrounding World War I, however, caused Americans to broaden their intellectual horizons, to tolerate the fact of the revolution, and to sift the results for an understanding of how Mexicans as individuals had been forged by their *circunstancia*.

Racism persisted as evidenced by Wallace Thompson (*The Mexican Mind: A Study of National Psychology* [1922]), but sensitive observers with social concerns, such as Carlton Beals (*Mexico, an Interpretation* [1923]) and Ernest H. Gruening (*Mexico and its Heritage* [1928]) discovered the Mexican to be a praiseworthy and creative person possessed of an appreciation for human existence that in most ways surpassed values being pursued in the so-called developed societies. This new-found appreciation for Mexican culture culminated in the work of Frank Tannenbaum (*The Mexican Agrarian Revolution* [1929]), who was undoubtedly inclined by his socialist tendencies but whose work remains among the most perceptive yet concerning Mexican character.

Professor Meyer principally treats the era from 1900 to 1940, although her epilogue ap-

proaches the present. In this final section she notes that a professionally trained corps of United States historians is producing a stream of monographs that approach the truth of the revolution—the truth being a more or less official Mexican viewpoint of its own revolution. In short, not only has the revolution itself become institutionalized, but also the manner in which United States historians view the event. Furthermore, the need to affirm United States values in light of the cold war, to contradict Soviet affirmations of a frustrated class struggle in Mexico, and, more importantly, to discredit the Cuban kind of social change has led American historians to label the Mexican model “the preferred revolution.”

The major shortcoming of the book is Professor Meyer's admitted failure to consider those United States domestic currents that have undoubtedly influenced much of the writing on Mexico. For instance, one better comprehends the interpretations of William E. Carson (*Mexico, the Wonderland of the South* [1909]) or Nevin O. Winter (*Mexico and Her People* . . . [1923]), when it is pointed out that the idea of white supremacy permeated the Progressive spirit. It also needs to be said that the Mexican Indianist movement of the 1920s coupled with the discontent of many American intellectuals toward their nation's emphasis on materialism influenced authors of the period to idealize Mexico's pastoral society. How their country's internal affairs affected the attitudes of United States writers toward Mexico is studied by Donald L. Zelman in his dissertation at Ohio State University, “American Intellectual Attitudes Toward Mexico, 1908–1940” (1969).

Despite the failure of Professor Meyer to develop fully her investigation, her book stands on its own as a pioneering effort to evaluate Mexican historiography from the Mexican point of view. Students of Mexican history can only agree that more of the same is needed.

PAUL J. VANDERWOOD  
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BERTA ULLOA. *La Revolución intervenida: Relaciones diplomáticas entre México y Estados Unidos (1910–1914)*. (Centro de Estudios Históricos, New Series, Number 12.) [México, D. F.:] Colegio de México. 1971. Pp. xi, 394.

Despite the cartoon on its cover depicting Tío Sam choking and kneeling *La Patria*, this is a work of careful scholarship with no discernible bias. In fact Miss Ulloa is less critical of Woodrow Wilson's Mexican policy than the North Americans Robert Quirk and Arthur Link. Her research is impressive, and she relies almost exclusively upon primary materials from Mexican and United States archives in presenting a straight, factual account of relations between the two countries between 1910 and 1914. She does not write as colorfully as Quirk nor with the impact of John Womack, but she has put together as complete a study of the events as may be found in any language.

There is probably more here of value to the Mexican scholar than to the North American. That is, most of the documents used may be found in United States archives (the National Archives, the Manuscripts Division of the Library of Congress, and manuscript collections of the University of Texas and Yale University), which, because of their availability, have already been researched to a considerable extent by U.S. scholars. Miss Ulloa does cite material from the archives of the Mexican Foreign Ministry, but the amount is slight and the ground seems familiar. Because much of the story is taken from U.S. documents, one may feel a “coals to Newcastle” effect. However, it is likely that Mexican scholars will derive a great deal more from a work of substance as this is in the original Spanish than from any comparable translated work.

Moreover, North American scholars will be disappointed if they look for a “Mexican viewpoint.” Miss Ulloa's judgment is balanced. One cannot detect even the “insidious” cultural influence that supposedly makes objectivity unattainable. On the other hand, the lack of an interpretative framework is a weakness of the book. Miss Ulloa permits herself to observe only that Woodrow Wilson wanted desperately to dominate Mexican political affairs and that he was ineffective. Further, she concludes that the military successes of the Constitutionals, not the actions of Wilson or the Niagara Falls Conference (to which she nonetheless devotes one of her longest chapters), were responsible for the fall of Victoriano Huerta.

This work is part of the project sponsored by El Colegio de México under the direction of Daniel Cosío Villegas to write a contemporary history of Mexico. The project includes the collecting and editing of documents and the publication of individual and group research. Miss Ulloa's volume lends real distinction to the project.

CHARLES D. AMERINGER  
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GEORGE WILLIAM PILCHER, *Samuel Davies: Apostle of Dissent in Colonial Virginia*. Knoxville: University of Tennessee Press, 1971. Pp. xi, 229. \$9.75.

Samuel Davies (1723-61) is best known as the "New Side preacher who consolidated the spontaneous revival in Hanover county Virginia being conducted by laymen whom Governor Gooch had christened 'Presbyterian'" and for his leadership of the successful legal battle for recognition of "the rights of dissenters under the English Toleration Act of 1689."

To these aspects of his career this biography adds little that is new. It does round out the picture of the man who during a career of less than fourteen years achieved a reputation as perhaps "the greatest preacher that America has known," whose oratory set an example followed by Patrick Henry, whose sermons (published posthumously in England in five volumes) went through more than twenty reprintings between 1766 and 1867, who was "America's first important writer of original hymns," who published poetry that was read throughout the colonies and was reprinted in 1968, who was recognized as one of the most eminent leaders of the Great Awakening, who pioneered in "the education of Negro slaves and . . . Indians," who was known as a foremost educator, and who for the last year and a half of his life was president of the College of New Jersey, for whose first permanent buildings he had spent fourteen months in Britain raising money.

Unfortunately Professor Pilcher's biography is a eulogistic extension of the funeral sermon given by Samuel Finley, Davies's successor at Princeton. The conclusion that his life "displayed a remarkable unity of inward belief and outward action" is contradicted by the evi-

dence. What emerges is the portrait of an oversensitive young man from a poor Baptist family turned Presbyterian, married into a prominent Williamsburg Anglican family, and struggled through a life beset by constant "feelings of inadequacy" and an inner turmoil that one may guess was related to his perennial ill health.

Structurally the portrait is a collage of material from and about Davies pieced together with Pilcher's uncritical observations. The resulting conglomerate calls attention to many apparent contradictions in Davies's career that are left unresolved and suggest that he erected a protective façade of pious deviousness. A few examples will make this clear.

Aside from having "seven [licensed] pulpits located in five different counties" Davies recorded that in two months he rode five hundred miles and preached forty sermons, often in the woods, while extending his work widely in Virginia and into North Carolina. Yet he staunchly denied that he was an itinerant. Pilcher agrees, saying that to Davies itineracy "meant going from place to place in a deliberate attempt to win converts," and he adds that this was something Davies "did not do," for he preached only "to congregations already converted." Yet it is also noted that he was a revivalist who always preached "to bring sinners to repentance." In this connection Pilcher seems uncritically to accept Davies's denial that he was a disruptive force in the colony, which a relative accused him of being, while also noting that he brought about "a mass defection from the established church."

Davies, says Pilcher, "was essentially a man of peace, preaching the words of a Prince of Peace" and admonishing his hearers to "cultivate a pacific temper towards one another, both as individuals and nations." Yet he preached preventive war in the name of defense, urged enlistment as a Christian duty, disobedience to which would bring God's curse, and "equated patriotism with Christianity and became, through his pulpit, the colony's best recruiting officer."

Davies's successful fight for the rights of dissenters, says Pilcher, helped "lay a firm base for the ultimate separation of church and state." Yet Davies, who favored Anglican bishops for



America, never questioned Establishment in principle and thereby threw his influence on the side of its perpetuation.

As for the education of slaves, Pilcher makes it clear that Davies's desire was to give them the ability to "read the basic texts of Christianity" in order that they might become church members, which he supposed, although he never told the slaves this, would "make them more loyal to their masters." Davies approved slavery in principle by holding slaves himself and, Pilcher weakly observes, "expressed whatever doubts he had about the institution by refraining from praising it." Actually his position suggests a Christian legitimization of slavery by separating being Christian from being free.

SIDNEY E. MEAD

*University of Iowa*

WILLIAM JAMES MORGAN, editor. *Naval Documents of the American Revolution*. Volume 5. *American Theatre: May 9, 1776–July 31, 1776*. With a foreword by RICHARD NIXON and an introduction by F. KENT LOOMIS. Washington: [Naval History Division, Department of the Navy.] 1970. Pp. xxviii, 1486. \$13.25.

I have been critical of the navy's documentary history of the Revolution in the past; if I continue to be so it is not out of sheer perversity. The project itself is of the utmost historical significance, yet it has never measured up to the high scholarly standards set by similar projects such as the Jefferson Papers, the Adams Papers, and the Franklin Papers. Its editorial practices have been faulted on many counts: the failure to state precise criteria for including or omitting documents, the tendency to reproduce documents from secondary sources when the primary sources themselves were available, the pronounced Whiggish bias in the written commentaries, and an index that has not always taken into account matters of current concern to scholars who are seeking different data as they resort to quantification techniques or to cross-disciplinary approaches in their research. Although the project has improved certain of these practices, it has yet to attain the level of competence in documentary editing that has been reached by other major editorial enterprises.

In the volume under consideration two major flaws are evident. First, there is the re-

production of numerous documents from Peter Force's *American Archives* (1837–53). Some of these sources, to be sure, have been destroyed or lost and can no longer be found in the original. But to rely so heavily upon the crude efforts of Force in these days of modern editing standards seems highly questionable. Second, there is an excessive reliance upon colonial newspapers for information. Much of this work was done by the project's former editor, the late William Bell Clark, who searched the press for shipping entries and prepared a voluminous file on the arrival and departure of vessels. Clark's file should have served as the focal point for further research into ships' logs. Colonial newspapers, after all, were no more accurate or detailed in their information than their modern-day counterparts.

Given the head start this project had with Clark's monumental collection of naval document transcripts, the financial backing of the federal government, and the availability of unemployed Ph.D.'s to serve as research assistants, scholars have every right to expect a higher standard of performance in this important historical undertaking.

GEORGE ATHAN BILLIAS  
*Clark University*

JOSEPH L. GRABILL. *Protestant Diplomacy and the Near East: Missionary Influence on American Policy, 1810–1927*. Minneapolis: University of Minnesota Press. 1971. Pp. x, 395. \$13.50.

In his 1968 presidential address John K. Fairbank challenged his colleagues in the American Historical Association to study "the missionary in foreign parts" whom he called "the invisible man of American history." Joseph L. Grabill has responded with this first comprehensive account of the influence of Protestant missionaries on the relations between the United States and the Ottoman Empire. The title is somewhat misleading. The bulk of the work deals with missionary efforts to guide the United States in remaking the Empire during and following the First World War and, more specifically, with the attempt to create an independent Armenia under American tutelage.

There is much to admire in this account. The research, at least up to 1919, is thorough and instructive, and some of Grabill's conclu-

sions are impressive. (He is probably right, for example, that the Armenian disaster could have been less had the missionaries' schools and hospitals been as open to the Turks as to their subjects.) Confessing himself in his preface to be an active Christian, he has sought to be impartial, and for a time the reader is persuaded that he has been. It appears, however, to at least one reader that the effort has cost him too much. He has leaned so far over to give the Turks their due that he has weighted his testimony against the Armenians, the missionaries, the Near East Relief, and Woodrow Wilson. The scarifying treatment of the American president in particular contrasts unpleasantly with the author's indulgence toward the bigoted Admiral Bristol, whose chief motivation was to make the new Turkey a happy hunting ground for American business.

Moreover, the very thoroughness of the research in its limited area may leave the impression that an independent Armenia guided by the United States was solely a conception of American Protestantism. The statement of the British ambassador (p. 230) that the Armenian mandate might still be put across by Morgenthau, Elkus, and Smith—two Jews and a Catholic—attests the inadequacy of such a view. Grabill has conscientiously set it down but seems not to have perceived its irony.

Worst is the account of the destruction of Smyrna in 1922, in which the statement of President MacLachlan of International College that the Turks not only committed no crimes but protected the college from destruction (by whom, one wonders) appears to have been credulously accepted. It is to be hoped that Professor Grabill will read Marjorie Housepian's excellently documented *The Smyrna Affair* and learn in harrowing detail that the Turkish army, far from maintaining order, set fire to the city and killed approximately 190,000 Greeks and Armenians by shooting, stabbing, fire, and deportation to the interior, while ships of the allied powers rode idly at anchor in the harbor; only the intervention of private individuals finally obliged them to save some of the victims. In this holocaust MacLachlan saw only that his school was spared. If Grabill had wanted to write a book against missionaries—as it sometimes seems he did—that would have been a good place to start.

The writing is generally serviceable, at times even making its points sharply, but some of it is awkward and some not even correct English. It does not appear to have profited from the kind of editing we ought to expect from a major university press.

In spite of these reservations the book should be read. Its author has ventured into unexplored territory and has done much of his research well. If he has fallen too often into error, I am persuaded he has done so from an excessive desire to be fair, of all intellectual maladies the least likely to spread.

JAMES B. GIDNEY

Kent State University

ALLAN NEVINS. *The War for the Union*. Volume 3, *The Organized War, 1863-1864*; Volume 4, *The Organized War to Victory, 1864-1865*. (*Ordeal of the Union*, Volumes 7 and 8.) New York: Charles Scribner's Sons. 1971. Pp. 532; 448. \$15.00 each.

These two volumes bring to a satisfactory though premature close the most ambitious undertaking of one of the most remarkable men ever to enter the ranks of historical scholarship. Publication of the series collectively titled *Ordeal of the Union* began in 1947, when Allan Nevins was already fifty-seven years old. His general plan contemplated as many as ten volumes on the three decades of the Civil War era from 1847 to the end of Reconstruction. At his death in 1971, he had completed eight volumes, carrying the story down to 1865. To scarcely twenty years of American history he had devoted about one and one-half million words—the equivalent of perhaps fifteen ordinary books. Yet even without counting this magnum opus Nevins would still be regarded as one of the most prolific and gifted of American historians. Indeed, the list of his other publications since 1947 would probably match the lifetime production of many well-known scholars. When one also considers all of his earlier works, including two Pulitzer Prize biographies, it seems safe to say that no other American has ever produced so much historical writing of such high quality. It is a record that may never be equaled.

The first four volumes of *Ordeal*, though panoramic in their subject matter, were primarily about the coming of the Civil War.

They appeared most opportunely at a time when Civil War causation was the focus of extraordinary scholarly attention and controversy. During the years 1942 to 1950 the problem of explaining the disruption of the Union was wrestled with in significant books by Avery Craven, James G. Randall, Roy F. Nichols, David M. Potter, and Kenneth M. Stampp. Debate between the so-called revisionists and their critics over the irrepressibility of the conflict had reached its peak. The Nevins volumes were the climactic contribution to this outpouring of literature on the reason why, and they remain, in Potter's words, "the only great overall narrative based upon modern research." Aiming at historical synthesis on a grand scale, Nevins tried to assimilate conflicting scholarly views into an interpretation that seemed, as a result, to be ambiguous if not contradictory. More often than not, he appeared to place himself squarely in the nationalist tradition of James Ford Rhodes, but at times his tone became revisionist as he condemned the "failure of American leadership" in the 1850s and asserted that the war "should have been avoidable." This eclectic approach caused much confusion among reviewers who found him fair and unfair in his treatment of the South, sensitive and insensitive to the moral aspects of the slavery question.

At the conclusion of his fourth volume Nevins echoed Rhodes in declaring that the war was fought over slavery. But not slavery alone, he added. It was also a war over "the future position of the Negro race in North America." This addendum, though untenable as an explanation of why the war came, has considerable validity as an explanation of what the war came to mean, and it anticipated the trend of scholarship in the years that followed. After 1950 interest shifted from the somewhat jaded subject of war causation to the seemingly more relevant subjects of racism, slavery, the Negro and his white champions, and the problem of why the Emancipation Proclamation became largely a "deferred commitment." Only by exploiting the racial theme fully and giving it primary emphasis could Nevins have made his later volumes as timely and fashionable as the earlier ones, but he did not choose to do so, perhaps holding something back for special treatment in his final volumes on Reconstruction.

Not that slavery and the Negro are neglected in *The War for the Union*, but they are only marginally related to the principal argument with which Nevins sought to give this sweeping narrative a general conceptual structure.

The theme of the war volumes, Nevins announced in 1959, was to be "the impact of the war on national character." More particularly: "Their thesis, insofar as a single idea can be applied to a struggle so manysided, is that the war measurably transformed an inchoate nation, individualistic in temper and wedded to improvisation, into a shaped and disciplined nation, increasingly aware of the importance of plan and control." This assertion, variously phrased, appears again and again like a leitmotif throughout the rest of the work. Antebellum America was "amorphous," "limp," "formless," "protoplasmic," "spineless." The war produced a "coordinated," "directed," "organized" nation. Reiteration is bulwarked with elaborate description, most notably in a chapter of the third volume entitled "The Sweep of Organization." The transformation, it is plainly implied, was overdue and beneficent, ushering in modern America, whereas at the beginning of the whole series Nevins had declared that the war left "part of the country . . . half ruined for generations, and all of it set back by decades." Definition of the word "organized" and its equivalents is never very clear. In some passages we are led to believe that there was a wholesale shift from individualistic to collective effort during the war years; on other pages the significant change becomes the nationalizing of organizations admittedly already well developed at local and regional levels. Similarly, we are told both that war produced the transformation with revolutionary suddenness and that the war merely accentuated or stimulated changes set in motion before 1860. Furthermore, although he asserted that the transformation was measurable, Nevins made no real effort to measure it. He provided enough data perhaps to justify an initial hypothesis but developed no design of proof.

The fact is that Nevins had too much peripheral vision and flexibility of mind for the relentless pursuit of a grand thesis, and his style of writing was not suited to such enterprise. History he once defined as an "integrated narrative . . . written in a spirit of critical in-

quiry for the whole truth." That the spirit was with him always can scarcely be doubted, but in his intensive search among the varieties, complexities, and ambiguities of history he found truth pluralized and seldom consistent. Even his more limited generalizations and judgments often have a two-way stretch. As for wholeness, in this series it became largely a matter of scope. One is impressed first of all by the vastness of his scale and the reach of his knowledge, but with reach there was also grasp and integrative understanding. Details are occasionally wrong and judgments are sometimes questionable, but reading Nevins gives one that sense of mastery which in public life is associated with statesmanship. The true measure of his achievement is what he made of the past, not what he abstracted from it.

Primarily narrative and descriptive, with interspersed passages of analysis, these two final volumes, like their predecessors, may be regarded as an updated, professionalized specimen of traditional literary history, and Nevins has perhaps carried that kind of scholarship about as far as it can go. To some readers his writing will also seem old-fashioned in another way. His outlook was that of a confirmed nationalist in the sense that he placed the nation at the center of his attention and associated the highest human values with nationhood. For him, as for Lincoln, the great conflict was above all a "war for the Union." He justified giving more emphasis to Northern than to Southern affairs on the grounds that the Confederacy was a temporary aberration. He had little sympathy for the Peace Democrats, labeling their 1864 platform "silly and evil." His pages vote overwhelmingly but not unanimously for Lincoln. In one long chapter, "The Tragic Lot of the Freedmen," he passed severe judgment on the Lincoln administration for its insensitivity and incompetence in dealing with this problem. Yet, on the political front he displayed a persistent hostility to Radical Republicans like Charles Sumner who were most concerned about the Negro's welfare.

As Civil War history, however, the Nevins volumes are by no means traditional. Military events, treated adequately though somewhat discontinuously and with wretched maps, are but part of the larger subject, which is the wartime nation. No doubt Nevins made his most

original contributions in those chapters detailing the effort behind the fighting, including such things as military transportation, industrial conversion, procurement of medical supplies, and the outpouring of war songs. These sections make heavy reading, laden as they are with names and statistics. But then there are other chapters on politics and diplomacy in which lively narration and deft character sketches quicken the pace again. Beginning with Vicksburg, "the organized victory," and Gettysburg, "the fumbled victory," these volumes continue past Appomattox and the tragedy in Ford's Theater to include two chapters on demobilization. It is a suitable ending, but there was plainly more to come, and one wishes, sadly, that it could have been so.

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WILLIAM N. STILL JR. *Iron Afloat: The Story of the Confederate Armorclads*. [Nashville:] Vanderbilt University Press. 1971. Pp. x, 260. \$10.00.

In his *Confederate Shipbuilding* (1969) William N. Still, Jr. gave us a brief, informative analysis of the difficulties that hampered Southern construction of warships, both wooden and ironclad. In *Iron Afloat* he concentrates upon the armor-clad vessels, telling why and how they were built and equipped, describing their battles in lively narrative style, and showing their contribution to the Confederate war effort.

Early in the war the enterprising Confederate secretary of the navy, Stephen R. Mallory, decided to offset the numerical superiority of the Union's wooden fleet by building iron-armored warships. In pursuit of this policy "approximately fifty ironclads were laid down or contracted for within the Confederacy, and twenty-two of these were commissioned and placed in operation." More might have been completed had not the Confederate shipbuilding program encountered chronic difficulties, such as shortages of iron and skilled labor, and inadequate transportation. In discussing these troubles, Still draws repetitiously upon *Confederate Shipbuilding*, reprinting a number of passages almost verbatim.

The ironclads were used mainly for river and harbor defense, and for this purpose Secre-

tary Mallory and his chief naval constructor John L. Porter favored casemated rams with inclined sides designed to deflect projectiles. Modeled after "the prototype Confederate ironclad," the *Virginia*, these warships were crude in appearance, slow and clumsy, and generally underpowered with inferior engines. Still defends them against critics who have emphasized their "makeshift" qualities. As warships, he argues, they were "potentially formidable," and Union naval commanders feared them. However, his own clear exposition of their defects helps fasten the term "makeshift" in the reader's mind.

His conclusion as to how much the ironclads contributed to the Confederate war effort is judicious. He points out that first the *Virginia* and later the James River squadron guarded stretches of the James against Union naval forces, that the *Albemarle* played an important part in the Confederate victory at Plymouth, North Carolina, in the spring of 1864, and that ironclads strengthened the harbor defenses of Savannah, Charleston, Wilmington, and Mobile.

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DAVID M. JORDAN. *Roscoe Conkling of New York: Voice in the Senate*. Ithaca: Cornell University Press. 1971. Pp. xiii, 464. \$15.00.

We have long needed a first-rate biography of Roscoe Conkling, for his political career was important not only to New York but to the nation during the Lincoln, Johnson, Grant, Hayes, Garfield, and Arthur administrations. Frustrated by a lack of relevant manuscripts (Conkling destroyed many of his personal papers), historians have been inclined to take the judgments and generalities of the haughty boss's many enemies at face value, and the result has been less than satisfactory. Donald B. Chidsey published a biography in 1935, but it was none too scholarly, and David M. Jordan labels it "a popular account."

Jordan is a practicing attorney in Pennsylvania who first became interested in Conkling as an undergraduate at Princeton. For a decade he worked, on his days off and vacations, on a full-scale biography of the senator. The

product, Jordan's first book, is an often useful but badly flawed work that, unfortunately, by its sheer bulk, may deter scholars from undertaking the thorough study that remains to be done.

The book has strengths. Jordan writes well (though the text is padded to a bewildering degree with common knowledge), he sketches the lives of a good many secondary political figures whose careers have been almost forgotten in recent decades, and he has an extraordinarily sophisticated view of James Garfield. He read widely in secondary sources, traveled to numerous archives, made admirable use of the Utica Public Library, and called attention to the importance of the Alonzo B. Cornell papers. But the volume's errors and faulty interpretations are legion, and one may wonder why the experienced editors at Cornell University Press, if not Jordan, were unable to catch them. Limited space permits mention of but a few.

Jordan's failure to examine New York newspapers in depth caused him virtually to overlook the Conkling machine's tight control of New York City Republicanism from 1871 to 1882. He did not carefully investigate the Phelps-Dodge case or the New York Customhouse controversy, and he badly misconstrues the Conklingite struggle with the Hayes administration. The account of Conkling's flirtation with Tilden during the postelection struggles of 1876 results in confusion. Jordan overlooks the significance of the Conkling-Lamar affair of 1879, and his portrayal of the senator's relationship to Katharine Chase Sprague after 1879 is unconvincing.

Part of the author's difficulties stemmed, apparently, from a hasty reading of documents—such as the Chester Arthur papers in the Library of Congress and the New-York Historical Society. Surely his amateurism hampered his effectiveness. At one point P. R. Levin's *Seven by Chance* (1948) is employed as documentation; on occasion one is soured by such sentences as: "The basic quality of American life was materially affected by the depression of the seventies" (p. 195). Horace Greeley is compared with Harold Stassen, James G. Blaine with Richard Nixon. And Jordan seems to have learned very little from several books he cites, such as Ari Hoogenboom's important *Outlawing the Spoils* (1961).



As a whole, this is the better of the two Conkling biographies, but it is hoped that historians will consider the task unfinished.

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HOWARD MUMFORD JONES. *The Age of Energy: Varieties of American Experience, 1865-1915*. New York: Viking Press. 1971. Pp. xix, 545. \$12.50.

*The Age of Energy* attempts to locate a "center" or "leading principle" in the period extending "from the age of Andrew Johnson to the presidency of Woodrow Wilson," an American era that by the author's admission "never really attained either political or cultural unity." Professor Jones's search for the meaning of the "extraordinarily diverse" forces contained within a "tumultuous republic" leads him to the principle of energy that in an extended conceit he applies to the life styles of American entrepreneurs, the restless cosmopolitanism of a new elite, and an aggressive eclecticism in the arts as well as to the creation of material abundance and the robust, unreflective tone of late nineteenth-century politics.

There are both advantages and disadvantages to the concept of energy as an organizing idea, the advantages stemming from its suggestive power and the range it affords the author's insatiable curiosity and lively concern for the cultural fact, whether it be the decor of Gilded Age sporting palaces, the menus of fashionable restaurants catering to gargantuan appetites, or the sudden rash of "How to" books signifying a renewed concern with craftsmanship. Yet if the strengths of Professor Jones's fascinating survey lie in the province of the descriptive, so too do its limitations—in the inevitable imprecision of the concept of energy as it is stretched to cover the bewildering variety of American behavior in these years. Thus to take merely one example, while it may be instructive in a limited sense to consider Walt Whitman and John D. Rockefeller as similar embodiments of the principle of energy, holding like visions of "a happy, wasteless, and plentiful society," the suspicion remains that they were, after all, two very different sensibilities with widely divergent systems of values. Something like this re-

servation writ large persists and constantly qualifies our assent to the argument—our conviction that variety, multiplicity, and idiosyncrasy ultimately triumph despite the author's case for underlying unity.

These doubts are confirmed when Professor Jones turns to the pragmatic sanction as exemplified in Theodore Roosevelt, the "magnetic center" of the age, subordinating "other and lesser conflicts" within it. The author eschews both the moralistic interpretation of "an age of excess" and the bureaucratic preferences implied in a "search for order." At its deepest level, therefore, *The Age of Energy* seems a response to personality, to that "amazing gallery" of powerful and picturesque figures—Annie Oakley, Ned Buntline, Adah Isaacs Menken, Daniel Drew, Jim Fisk, John D. Rockefeller, Andrew Carnegie, and Theodore Roosevelt—in whom various forms of energy come to concentrate. What held American society together in these years and supplied its citizens with such great vitality, Professor Jones argues, was the widely shared belief that ultimate meaning is determined by practical consequences and that "you can affirm any two things as good if they 'work' until you find in practice that they cancel each other or that something else or some other pair of opposites 'work' better." The question for the historian, however, is when and, more particularly, how such a handy rationalization occurred to post-Civil War America and whether conceiving of individual choices and decisions as comparable or even contradictory expenditures of energy really illuminates an age.

These questions enmeshed in the central concept of the study are mirrored in the form of the book, which is a series of personal essays or lectures with which Professor Jones has delighted generations of students of American culture. His presence and voice are constant, and what we see most clearly is the process of a vigorous mind at work on unshaped material, asserting and explaining a method, qualifying but making generalizations forcefully, discovering, quoting at length, cataloging, and combining. The result is a kind of "action history" of an intellectual engagement with the scaffolding intentionally left standing. If the final effect is that of a set of "notes toward a definition of culture" rather than a satisfying synthesis, we

nevertheless sense that here is much the same sort of energy that the author so admires in the people of the Gilded Age.

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LAWRENCE D. RICE. *The Negro in Texas, 1874-1900*. Baton Rouge: Louisiana State University Press. 1971. Pp. ix, 309. \$10.00.

ARLEN L. FOWLER. *The Black Infantry in the West, 1869-1891*. Foreword by WILLIAM H. LECKIE. (Contributions in Afro-American and African Studies, Number 6. Negro Universities Press Publication.) Westport, Conn.: Greenwood Publishing Corporation. 1971. Pp. xviii, 167. \$9.50.

Although both these books deal with Negroes in the West during brief periods of the post-emancipation generation, there is no topical overlapping. They both manifest to some extent the understandable desire of the doctoral candidate to limit his coverage as much as is permissible.

The Rice volume is basically a political history of Negroes in the black belt and major cities of Texas from "Redemption" to the disappearance of the Negro as a political factor, with background attention to his economic, social, and cultural situations—a geographical emphasis that excludes not only Negro soldiers and trail drivers but also the expulsion in 1886 of the entire Negro population of Comanche County. One can sympathize with Dr. Rice's wish to avoid "the labyrinth of the Reconstruction story," but it is difficult to understand the Negro's political situation after "Redemption" without more knowledge than is available of his position during Republican dominance. The Fowler volume, too, does not begin as early as one might expect, inasmuch as black infantry regiments such as the 62nd, the 116th, the 125th, the 38th, and the 41st were represented in Texas, Kansas, and New Mexico prior to 1869. (On the other hand, unless the author intended to do more than refer to a secondary work, it was ill advised to carry the story beyond 1891 to include the grisly and tragic Houston mutiny in 1917 by a battalion of the Twenty-fourth.)

*The Negro in Texas* is a depressing narrative. The political elimination of the Negro was achieved by measures ranging from eco-

nomic pressure and the poll tax to expulsion, whipping, and murder. Not until 1966 did a Negro again sit in the Texas legislature. "Jim Crow" coaches were introduced as early as 1866. Although "Texas made greater progress in reducing Negro illiteracy than any other southern state," other cultural and economic developments were not impressive. The discussion of the Negro press might, however, have included mention of Emmett J. Scott, editor of the *Texas Freeman*, 1894-97, and later Booker T. Washington's confidential secretary. Rice's book contains no illustrations and, inexplicably, no map.

Fowler's volume on the two black infantry regiments, the Twenty-fourth and Twenty-fifth, supplements W. H. Leckie's study of the Negro cavalry in the West (1967). Their role was unspectacular but indispensable. While the dashing Ninth and Tenth Cavalries rode to the relief of beleaguered white troops and struck unexpected blows at hostile camps, the black footsloggers built roads, guarded wagon trains, and garrisoned posts; when they did see action, however, they demonstrated their worth. Their enemies were not merely Indians and outlaws but also "racial prejudice and discrimination." Ironically, it was after the Twenty-fifth was transferred from Texas to the North that it lost two soldiers by lynching. On the other hand, it enjoyed heart-warmingly friendly relations with the people of Missoula, Montana, achieved largely although not entirely through its magnificent regimental band. Two generations later, old-time Missoulians still remember this black regiment with affection and respect.

As contributions to a historical field—the Negro in the West—in which until recently little has been done, both these volumes will prove highly useful. The Rice volume, in particular, also points up how much still remains to be done.

KENNETH WIGGINS PORTER  
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KENNETH S. LYNN. *William Dean Howells: An American Life*. New York: Harcourt Brace Jovanovich. 1971. Pp. 372. \$13.95.

A major reason for Professor Lynn's new biography of Howells is to refute the conventional view of Howells as a complacent author writ-

ing about the complacent society of late nineteenth-century America. Taking "Leslie Fiedler's scornful characterization of Howells. . . as 'resolutely cheerful, progressive, and sane'" as representative of this critical perspective, Lynn attempts to establish the contradictory view that "actually Howells was suffering . . . from a personal and artistic despair that was every inch as profound as the more celebrated glooms that gripped Henry Adams, Henry James, and Mark Twain. . . . As it did to his three most gifted literary contemporaries, the *fin de siècle* sometimes seemed to Howells like the end of the world."

But Professor Lynn goes beyond the suggestion that Howells suffered from the despair of his literary contemporaries. He links Howells with the existential despair of all modern men. "Howells," he writes, "was a man of modern sensibility, whose awareness of life was rooted in radical doubt and anxiety. All his life he was afflicted by a sense of aloneness, emptiness, and the precariousness of his personal being. . . . In the climactic psychological breakdown he suffered in the mid-1880's, he was overcome by the feeling that all his life he had been playing roles, and that as an influential man of letters in Boston he was fulfilling an outlander's ferocious ambitions at the cost of cutting himself off from his deepest emotional needs."

Professor Lynn is persuasive in his argument that Howells suffered from "radical doubt and anxiety." His book examines Howells's youth in great detail. He uses this biographical context to illuminate Howells's first great novel of social realism, *A Modern Instance*. Published in 1881, it revealed Howells's profound sense of alienation as the central American experience. Howells had become a compulsive worker by the time he was an adolescent. He was willing to sacrifice friendships and the family he loved in order to fulfill his ambitions. He made this ruthless search for success the controlling motivation of Bartley Hubbard, the young hero of the novel, who abandons his wife Marcia to become a free man without responsibility in the West. Professor Lynn feels that Howells uses the figure of Marcia to express his insights into the sexual inhibitions and repressions of his America and the peculiar father-daughter relationships that developed out of this sexual situ-

ation. For Howells the setting of the novel, the New England town Equity, is representative of the absence of social identity in America that dooms Bartley and Marcia to a life without meaning other than their own selfish instincts.

By the middle of the 1880s Howells was searching for some new sense of social meaning, and he was attracted to the utopianism of Tolstoy. But Lynn finds that while Howells was able to borrow the broad social realism of Tolstoy's *War and Peace* for his great novel of 1890, *A Hazard of New Fortunes*, he could not imitate Tolstoy's commitment to peasant life.

For the historian of ideas, the methodological problem of Professor Lynn's book is that while he succeeds in relating the ideas in Howells's writings to his biography, he does not relate Howells's biography to American or modern society. Professor Lynn has described Howells's as one of modern existential despair. But Lynn has not defined what he means by modern. Is there a peculiar relationship of the artist to society in the nineteenth century that leads most artists to a sense of existential despair? Does the middle-class family in the nineteenth century tend to create unhealthy personal relationships? Is Howells's sense of a national identity crisis in the 1880s representative of much of the American middle class at that time? Is the American national identity crisis part of a larger crisis of middle-class identity everywhere in modern civilization? The traditional form of the literary biography does not make it possible for the intellectual historian to address himself to questions like these.

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RICHARD W. SCHWARZ. *John Harvey Kellogg, M.D.* Nashville: Southern Publishing Association. 1970. Pp. 256. \$5.95.

This is a straightforward and thorough study of the public life of John Harvey Kellogg (1852-1943), a man of unbelievable talents and energies and a man of tremendous influence on American life. A power in the Seventh Day Adventist Church for about half a century, Kellogg was a well-known and skillful surgeon, a prodigious and popular writer on religious,

medical, and popular health topics in journals and books, an innovator and educator in the health fields, a successful public speaker well known in the Chautauqua circuit, a big business man, a philanthropist, and more.

The central theme of his life was what he called "biologic living." This was a tenet that came right out of the Seventh Day Adventist movement, and the movement became the fulcrum from which Kellogg launched his many enterprises. Biologic living entailed a vegetarian diet and abstinence from alcohol, tea, coffee, and cocoa. It included also fresh air, exercise, and cleanliness. The Battle Creek Sanitarium was his monument to these ideas. But most important in the pursuit of his many ideas and fads in the dietary field, Kellogg developed the process of flaking cereal and thus not only was the dry cereal industry of Battle Creek begun (the Kellogg involved in the tremendous expansion of the industry, however, was John's brother, William), but the breakfast habits of the American population were changed. John Kellogg, too, was the inventor of peanut butter and, it is also worth noting, of a mechanical exercise horse.

All of the details are to be found in Dr. Schwarz's book, which, unfortunately, does not provide the student with documentation. Instead he is referred to the dissertation at the University of Michigan upon which this book is based. The reader might find much more convenient the comprehensive list of sources given in Gerald Carson's *Cornflake Crusade* (1957).

More important, however, is the fact that one does not come away from this book with any sense of understanding the personality of its hero or of the intellectual forces that turned him from a fundamentalist into something of a pantheist and a believer in organic evolution. We are never quite sure how much of Kellogg was showmanship, egotism, and avarice for money or for power and how much was genuine Christianity and humanitarianism, although it must be admitted that such profiles would be difficult to draw. The Kellogg story is, moreover, too often isolated from the main-streams of American life. In short, like Dr. Kellogg's "Protose"—a meat-like vegetable protein food he developed—the book is substantial, in-

teresting, filling, but somehow not quite satisfying.

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RICHARD D. WALTER. S. Weir Mitchell, M.D.—*Neurologist: A Medical Biography*. Springfield, Ill.: Charles C. Thomas. 1970. Pp. viii, 232. \$9.75.

Dr. Walter, himself a neurologist on the staff of the UCLA Center for Health Sciences, has written a fourth biography of S. Weir Mitchell, who was certainly the most celebrated and perhaps the most important neurologist and psychiatrist practicing in late nineteenth-century America. The subtitle of this book, "A Medical Biography," helps to clarify its relationship with Anna Burr's *Weir Mitchell: His Life and Letters* (1929), Ernest Earnest's *S. Weir Mitchell: Novelist and Physician* (1950), and David Rein's *S. Weir Mitchell as a Psychiatric Novelist* (1952).

Dr. Walter's introductory statement, which amounts to a description of his biographical methods and purpose, clarifies even further: "the present 'biography' resembles in many respects a volume of collected works. The biographer has served more as an editor, and the frequent quotations and excerpts from the original make this apparent. It is hoped that this direct exposure to Weir Mitchell will prove more interesting and helpful to those training in the fields of neurology, psychiatry, and neurosurgery than the more traditional strategy of paraphrase and pontification." Presumably this "biography" takes its place in a series including *Sir William Osler: Aphorisms from His Bedside Teachings and Writings*, *The Unique Legacy of Doctor Hughlings Jackson*, *Early Days in the Mayo Clinic*, and *Garrison's History of Neurology*.

Weir Mitchell's reputation was established in 1864 with the publication of *Gunshot Wounds and Other Injuries of Nerves*, which grew out of his hospital service as a Civil War surgeon. His fame came with the "Rest Cure" in the mid-1870s, and his celebrity was consummated in 1897 with the publication of *Hugh Wynne*, which Thomas Bailey Aldrich put in tandem with *The Scarlet Letter* with his pro-

nouncement that they were the "two great American novels."

Mitchell was a strikingly accomplished and versatile man in whom many of the currents and tensions of Victorian America are vividly illustrated. Women's liberation polemicists could have a field day with him (but only by side-stepping the paradox created by the obvious fact that he was eminently successful with his women patients); his series of medical handbooks written for laymen—which are, among other things, commentaries on the social bases of psychic disorder in America—are worthy of monographic study themselves; his test of William Osler with the cherry pits raises intriguing questions about the interrelationship between etiquette and professional performance at a time during which the professionalization of the professions was occurring. That merely begins a list of rich possibilities in Mitchell's life awaiting exploitation by a biographer who wishes to make some splendid triangulations between a man, his milieu, and post-Civil War American society. Should such a biography be written, quite likely it will be as much a study of social ideas and values as an account of medical and literary achievements.

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THOMAS PARKE HUGHES. *Elmer Sperry: Inventor and Engineer*. Baltimore: Johns Hopkins Press. 1971. Pp. xvii, 348. \$15.00.

Professor Hughes's *Sperry* is not only an intensely interesting study of a highly competent and inventive American engineer, it is also a model of the publisher's art. Both Hughes and the Johns Hopkins Press are to be congratulated upon the excellent way in which both the textual and graphical materials have been presented. The use of a large format with wide margins that have allowed the presentation of notebook pages, patent drawings, letterheads, and other memorabilia illustrating the points being made in the text is a boon to the reader. One of the advantages of the person Sperry was and of the business in which he was engaged, which required tremendous documentation in order to fight patent interference suits, was that he left thousands of letters, sketches, notebooks, and the like. Thus it would be assumed

that it would be easy to document his career as inventor, scientist, entrepreneur, and maker of machines, processes, and systems. Even so, Hughes quite often has to use terms such as "about" and "around" in reference even to years because he has not been able to find exact dates.

Elmer Sperry is one of the cluster of Americans produced by the Industrial Revolution who made their names as individuals before specialization and corporate group research and development usurped the field. Sperry lacked a college education, though he attended some lectures at Cornell. But he had an intuitive genius for seeing where the weaknesses in others' ideas lay, for examining patents to see what they did not cover, and for the conception and engineering development to production of his own solutions.

Sperry had one great inherent advantage over his rivals in that he learned early the necessity for feedback, and whether he was controlling electric current for arc lights in the 1880s or auto pilots in the 1920s, many of the same principles were applied. More than this, Sperry was a practical entrepreneur. He developed those ideas for which he could get capital and sold them, taking royalties and consultancies when they were developed. In general his interest in a field lasted about five years, and he moved out as soon as big business moved in.

Students of American history, whether they are in economic, technological, or general history, should read this book for an intimate understanding of the position of the inventor-entrepreneur from 1880 to 1930. They will find that the reason Sperry teamed up with the navy in the development of gyro-stabilizing was not that he was or wanted to be a warmonger, but rather that since the merchant marine was not willing to capitalize research and development, he followed his natural instinct for going where money could be obtained to further the work in which he was interested.

If one can suggest any criticisms of *Sperry*, they lie in the failure to integrate the story more fully into the general trend of developments in the United States and elsewhere and that the aeronautical terminology is weak, perhaps from being too close to Sperry's notes.

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GLENDON SCHUBERT. *The Constitutional Polity*. (The Gaspar G. Bacon Lecture on the Constitution of the United States, 1968.) Boston: Boston University Press. 1970. Pp. xviii, 195. \$6.50.

This book by Glendon Schubert, a distinguished professor of political science long recognized as the dean of the behavioral school of judicial analysis, well illustrates the advantages and the weaknesses of behaviorism as a technique for studying the Supreme Court. Originally prepared as the 1968 Gaspar G. Bacon Lecture on the Constitution (sponsored by Boston University), the book represents Schubert's synthesis of behaviorist findings on the role of the Supreme Court in policy making since 1921. (Conventional analysis is reluctantly accepted by Schubert wherever the behaviorists have not yet trod.) Schubert devotes separate chapters, each covering the 1921 to 1969 time span, to policy "output," ideological "input," and the responses to Court policy. Inevitably there is considerable overlap and much of the material is familiar, but the book still makes absorbing reading.

Perhaps Schubert's most valuable analysis is on the 1930s. Combining simple calculations with sound historical perspective, he underscores the great divide of 1937. He finds the Court at the commencement of the struggle with the president "every bit as devious and dissimulative" (p. 28, referring to the *Hot Oil* case) as Roosevelt would be two years later. On the switch in 1937, Schubert quite properly refuses to be taken in (unlike other constitutional authorities) by Roberts's "self-serving memorandum" that attributed his change of vote on the minimum wage cases to a legal technicality. Schubert concludes that *all nine* of the pre-Black justices, including the "Wilsonian-Progressive" liberal bloc, were too conservative for the unfolding New Deal. Roosevelt's attack on longevity thus had real point to it.

Interesting observations abound in the book, usually based on vote counting in conjunction with astute analysis. Two examples are the chance effect of John H. Clarke's premature resignation in 1922, which deprived the Court of its "very first modern liberal," and the odd consequence of the Frankfurter-Harlan desertion of the liberal bloc in 1959, which left freshman Justice Stewart in the crucial middle position until Frankfurter's retirement in

1962. On the Warren era Schubert is highly favorable, especially to the great period from 1963 to 1969, when the Warren Court gave to many and diverse civil libertarian causes a "degree of favorable support" probably unequalled by any other court in the history of the world.

The basic limitation of Schubert's book (and even more so of behaviorist studies in general, which usually lack Schubert's sense of history) lies in its one-dimensional view of the justices and their work on the Court. By derivation the "input" side of Schubert's mix must be taken with considerable skepticism as a satisfactory explanation of change in the role of the Court.

Scorning on the one hand "legalistic doctrine and opinion verbiage" and similarly patronizing, if more respectful, to the nonquantified scholarship of Alpheus T. Mason, Walton Hamilton, and the like, Schubert stays relentlessly with the typical behavioral categories of liberal and conservative, subdivided into economic and political. Within these categories the justices stand emplaced with their "policy preferences" (little more than Pavlovian responses, it would appear from some behaviorists). That a justice may have a constitutional philosophy that will lead him at times contrary to his policy preferences or that a justice, because he accepts settled law, will indeed vote many times (usually on unanimous issues) contrary to his private wishes are seemingly inconsequential alternatives to Schubert and his school. The analysis makes no pretense of even considering the constitutional legitimacy of a justice's position. He is simply scaled on the chart and his "inconsistent" votes labeled deviations.

I would be the last to deny the influence on a justice's decisions of his result preferences (my own work has certainly emphasized judicial attitudes and values). The broad syndrome of judicial conservatism versus judicial liberalism is clear enough in any term of the Court. Nor has my current association with the lawyer's approach particularly endeared me to professional legalism. But it is impossible to explain Frankfurter without serious consideration of his intellectual convictions in regard to judicial self-restraint—constitutionally wrong, pedantically dogmatic, and ultimately oppressive as I believe they were. And can Harlan be

dismissed as simply a low-ranker on the liberal-conservative scale when we consider his skillful sensitivity and occasional decisiveness in such opinions as *Yates v. United States* (emasculating the conspiracy-to-advocate clause of the Smith Act) and *N.A.A.C.P. v. Alabama* (protecting the NAACP against state harassment and establishing freedom of association as a viable constitutional right)? One would never guess from Schubert's analysis that justices make constitutional law as well as public policy.

Schubert, of course, does not deny the varied institutional functions of the justices but asserts in the preface that his concern is simply with their input on policy. The result, however, is a grossly skewed view of the Court and the individual justices, the more deplorable since behaviorism is so much the fashion in political science departments.

It is in the consideration of Hugo Black's last years on the Court that Schubert's approach really runs aground. On the positive side, he makes effective use of case-vote compilations to drive home what we all sensed—that Black had sharply plummeted on the liberal-conservative scales. In the 1968 term Black became “the Warren Court's anchorman on civil liberties, dissenting forty-one times (twenty-five of these alone) against the sixty-eight split pro-civil liberties decisions in which he participated” (p. 118). To Schubert this precipitous drop bears the mark of “cultural obsolescence” and “psychophysiological senescence” (*sic*). Refusing to accept Black's strict constitutional literalism at face value, he accuses Black of shifting his “ideology . . . to remain in touch with his explicit conservative policy goals.” In the course of his indictment, Schubert misstates an important Black opinion (p. 189, n. 52), misinterprets the 1964 sit-in decisions, and describes Black as “leader of the die-hard opposition to the Court's expansive policy of racial egalitarianism” (p. 119). To anyone who has noted Black's powerful language in 1969 as Circuit Justice for the Fifth Circuit when he set the groundwork for *Alexander v. Holmes* (the Burger Court's decision outlawing “all deliberate speed”), Schubert's charges must seem cruelly unfair.

True it is that Black's innate traditionalism, like that of many another liberal gentleman, quickly surfaced in the mid-1960s in reaction

to the shock waves of increasingly militant protest, ghetto rioting, and the dramatic polarizing of generations. Yet even then Black's opinions were still grounded in constitutional formalism (even the Vietnam arm band opinion, painful reading though it is) against a backdrop, it must be remembered, of unprecedented judicial activism. At bottom Schubert's accusations are reduced to a complaint that Black's “policy outputs” were too conservative for the times.

I have other quarrels with the book, such as Schubert's prejudicial treatment of radicalism when he contrasts it to conservatism and liberalism, and his casual labeling of Justice Douglas as a “spokesman for radicalism.” There is no index, no bibliography, no table of cases; and the footnotes are at the back without benefit of page references at the top.

Despite these criticisms, *The Constitutional Polity* is well worth reading. It has much to say, and in compressed space too, about the Supreme Court since World War I. Schubert's writing is smooth, lucid, taut; even his statistics are handled gracefully. What a shame that so perceptive and mature a scholar should feel bound by the thesis that one must make a measurable science out of human (that is, judicial) behavior.

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R. ALAN LAWSON. *The Failure of Independent Liberalism, 1930-1941*. New York: G. P. Putnam's Sons. 1971. Pp. 322. \$7.95.

The title of this study does not clarify its contents. It is an intellectual history of a diverse collection of social critics of the 1930s who operated somewhere in the unchartable area between the New Deal and doctrinaire Marxism. Lawson calls his thinkers “independent liberals” (there are ten to twelve major ones, but Lawson does not confine himself to them), and his problems begin at once. The men seem independent enough, but they were certainly not all liberals, even in the broad meaning of the term. Note the leading figures: John Dewey, Lewis Mumford, Howard Odum, Horace Kalen, Louis Adamic, George Soule, Stuart Chase, Alfred Bingham, Lewis Lorwin, Herbert Agar, Allen Tate, Archibald MacLeish, and Waldo

Frank. Lawson grapples resourcefully with their diversity. He discerns a group of "pragmatic rationalists" whose guide was experimental reason and who usually favored some sort of collectivist planning. Then there were the "liberal traditionalists," men who would have preferred a society and social policy that strengthened organic social ties; the latter are further subdivided into "regionalists," such as Mumford, Odum, and the Southern agrarians, and "cultural pluralists," such as Kallen and Adamic. Later Lawson adds the "Great Tradition" spokesmen as another subcategory—men such as MacLeish and Waldo Frank.

I do not, it must be clear, find Lawson's organizing categories successful. Categories and labels multiply until they are often a hindrance, not a help. The book is further fragmented by an episodic organization in which men are analyzed in brief sections, often several times, and there are occasional discussions of the appeal of fascism, literature in the 1930s, the cooperative movement, and other subjects whose relation to the argument is not always clear. Even the word failure does not unify these men, for, as the author admits, they failed only to secure their social goals. Actually, he finds of most of them that they "analyzed cogently, often profoundly, and were not discredited by events."

Despite these flaws, the book is valuable. The intellectuals who remained critics of the dominant liberalism of the 1930s have long needed analysis, and Lawson is a perceptive, thoughtful critic. He has given us our first historical study of the careers of Stuart Chase and George Soule, who were not only impressive social commentators but men of some intellectual influence. Lawson's extended treatments of Dewey and Mumford are substantial contributions. The parts of the book add up to more than the whole, for it was impossible in the end to make a whole out of these careers.

There are no startling conclusions here, but students of the 1930s will be interested to note, for example, the shift in mood and focus that came to all the "independent liberals" after 1936 with the popular endorsement of the New Deal and the approach of war between totalitarian states. At the end of the volume Lawson is discussing Paul Goodman, a characteristic diversion in a pleasantly meandering book. But

along the way we have had new insights into some familiar and a few unappreciated intellectuals. The Allan Nevins Prize was granted the book not for its organizational mastery, one assumes, or because it is entirely free of opaque phrase, but for the general gracefulness of its style and the balance of its judgments.

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Santa Barbara

CHARLES F. O'BRIEN. *Sir William Dawson: A Life in Science and Religion*. (Memoirs of the American Philosophical Society, Volume 84.) Philadelphia: the Society. 1971. Pp. vii, 207. \$3.00.

John William Dawson is a significant nineteenth-century Canadian geologist, educator, and author worthy of a first-rate biography. Charles F. O'Brien's work is basically an intellectual history treating Dawson as a controversialist. Thus the reader should be forewarned that O'Brien's book, despite its title, is not a detailed account of Dawson's career either in science or religion, but of his participation in several nineteenth-century scientific and religious controversies. The disputes considered are geology and Genesis, polygenism, evolution, the *Eozoön* controversy, and the issue of continental glaciers versus icebergs in accounting for the effects of glaciation. With the exception of the last issue, O'Brien views these disputes as having a rational connection to Dawson's overriding interest in combating evolution and in presenting "his Paleyite approach to science." And O'Brien notes with appropriate insight that "the key to Dawson's career as a controversialist was his use of Canadian materials to enter the mainstream of nineteenth-century controversy." In sum, O'Brien shows an understanding of the complex scientific and religious issues in dispute and traces with care Dawson's and his opponents' views. The result is a contribution to the history of science.

In justifying his study, O'Brien maintains that Dawson's "historical relevance in carrying on the search for a synthesis of the two theologies—natural and revealed—is much greater than his contributions to either education or natural science." One has the feeling that the wish is father to the thought, and the evidence

presented or omitted nourishes a skepticism of O'Brien's thesis. While one may portray Dawson as a man applying a keen mind to a losing cause, the resulting portrait is unfinished. To diminish his contributions to geology in general and paleobotany in particular, to slight his administrative abilities and successes in the development of McGill University, and to treat less significantly his share of professional scientific honors—a not unreliable indication of his scientific reputation—requires further reflection upon Dawson's historical importance.

In his "Notes on the Sources," O'Brien wisely recognizes the quality of A. Hunter Dupree's *Gray* and Edward Lurie's *Agassiz*; it is to be lamented that he did not follow that tradition in providing a well-defined portrait of Dawson.

WALTER L. BERG

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DARCY RIBEIRO. *The Americas and Civilization*. Translated from the Portuguese by LINTON LOMAS BARRETT and MARIE MCDAVID BARRETT. New York: E. P. Dutton and Company, 1971. Pp. 510. \$15.75.

Darcy Ribeiro, a Brazilian anthropologist, might better have entitled this book *Latin America: Victim of Imperialism*. Decked out in the traditional trappings of scholarship (footnotes, bibliography, and scatterings of social science jargon), the work presents a politically partisan attack rather than a scholarly analysis. Using a historical approach within a Marxian ideological framework, the author sets out to investigate the causes of the material progress of the United States and the economic backwardness of Latin America. He concludes that imperialism is the sole cause. Even historians who subscribe to such a simplified view are likely to be disappointed in Ribeiro's ineffectual attempts at historical proof.

The general tone of the study is quickly set in the portrait of Iberian civilization, which is described in the crudest terms of the Black Legend. "Terrorism," "oppression," "torture," "salvationistic fanaticism," and "plunder" are typical of the terms applied by Ribeiro to the Latin American colonial regimes. The author's credibility in the field of history may possibly be measured by his charge that "viruses, bacilli,

and germs" were used as "one of the decisive weapons of the conquest."

The wicked empires of Spain and Portugal are contrasted to the benign imperial constructions of the Aztecs and the Incas. The author glosses over the bloody and forceful processes of Indian imperialism and would have us believe that these theocratic empires "crystallized." Slavery and serfdom in Aztec society, so obnoxious to Ribeiro in Europe, are depicted as merely parts of a "stratified social structure." The practice of human sacrifice on a lavish scale is passed off as part of a world conception in which the Aztecs were supporters of "life and prosperity for all." The author's failure to mention the Inca custom of *mitima* or *mitimaes*, by which large population groups were uprooted and moved hundreds of miles for the convenience of the empire, is curious in the light of his denunciation of European nations for allegedly using colonists as human cattle. Obviously a double standard is applied in the judgment of European and Indian empires.

The depiction of Anglo-American history in this work is an outlandish caricature that abounds in factual errors and naive interpretations. To cite only a few egregious examples, Ribeiro states that English colonists came to America because they were displaced by the expansion of cattle raising; independence was "won as a project of whites of the north, splitting the nation . . . in the fight to abolish slavery"; and the Founding Fathers, during the American Revolution, sought to monopolize the colonial exploitation of Latin America. Since the early days of nationhood, we are told, the United States has engaged in an international plot to retard the development of the rest of the hemisphere. The current feeble policy of the United States of fostering world population control is seen by Ribeiro as a sinister "power policy aimed at continuing the dependence of Latin America."

Those interested in a serious, scholarly treatment of the negative influences of imperialism on Latin America will be well advised to look elsewhere. The writings of Víctor Raúl Haya de la Torre and Manuel Ugarte are more informative in many respects on the subject of imperialism. The chief utility of Ribeiro's study lies in its contemporary presentation of

the anti-imperialist views of the political Left in Latin America.

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NORMAN NEWTON. *Thomas Gage in Spanish America*. (Great Travellers.) New York: Barnes and Noble. 1969. Pp. 214. \$4.75.

Thomas Gage's perceptive but malicious description of life in seventeenth-century Spanish America was, and still is, controversial. To read it judiciously, one must know the author well, and Norman Newton's book is adequate for this purpose. However, it is no substitute for Gage's original *The English-American: His Travail by Sea and Land* (1648), nor for its scholarly versions edited by Arthur P. Newton (1928) and J. Eric S. Thompson (1958). Rather, this is a popularization, an artful paraphrase and condensation of the original text flavored with a few direct quotations and extended with additional information on Gage and his milieu.

In this treatment the protagonist emerges as something of a scoundrel. Although trained by the Jesuits, Gage became a Dominican; having volunteered for missionary work in the Spanish Philippines, he deserted on reaching Mexico and fled to Guatemala. There, first as a college teacher and then as an Indian village priest, he amassed a tidy fortune in twelve years, but during his unauthorized return to England in 1637, he lost it all to pirates. In troubled England he renounced his Roman Catholic religion and then his Anglican faith. Espousing the Puritan cause and conspiring against his Catholic family and friends, he published his vindictive travel memoirs (as propaganda for Oliver Cromwell's "Western Design"), and served as a chaplain in the English conquest of Spanish Jamaica in 1655. There he died the following year.

Newton makes no attempt to whitewash Gage's unsavory acts, but neither does he accept the persistent allegation that Gage, while serving as a friar in the Spanish colonies, was actually a spy for the English government.

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CELSO FURTADO. *Economic Development of Latin America: A Survey from Colonial Times to the Cuban Revolution*. Translated by SUZETTE MACEDO. (Cambridge Latin American Studies, Number 8.) New York: Cambridge University Press. 1970. Pp. xvi, 271. Cloth \$10.50, paper \$2.45.

Celso Furtado is one of a small group of Latin American economists with a substantial academic reputation in and beyond Latin America who have held important policy-making positions and whose writings have been widely circulated in English. His best known monograph—at least to historians—is *The Economic Growth of Brazil: A Survey from Colonial to Modern Times* (1963). The similarity in the subtitles of this work and the book under review requires comment because in contrast to the Brazilian study, this book is not a survey in any historical sense. The colonial period is disposed of in pages 8–18 and the nineteenth century is covered in the following fifteen pages. The remaining 260 pages of text deal with the twentieth century, effectively with the years since 1929. A sense of the past as an integral part of the present, however, pervades the whole study, and so in introducing topics such as agrarian reform or the Cuban Revolution, Furtado provides succinct and provocative summaries of past events designed to explain why things now happen as they do and how the past conditions present-day political responses to economic pressures.

Furtado is the most prolific and one of the more persuasive proponents of the position that effective analysis of development requires special formulations to fit various historic processes if it is to be precise and pertinent. The past, however, can be viewed differently by economists as well as by historians. His readable expository style as translated by Suzette Macedo makes it possible for the noneconomist to bring Furtado's vision easily into focus. The obvious danger is that such a person might accept his arguments without any awareness of alternative economic approaches to the situation. His is a historically informed and rationally ordered, if somewhat evangelical, mind at work—the founding rock of the church in this instance being the Economic Commission for Latin America.

The gospel of this text is that external forces are primarily responsible for the dependent



state of Latin America's economy and that national planning must be in response to this situation. A few of the many examples cited in their specific national and regional historical context are unfavorable terms of trade for agricultural and mineral exports; the extent to which such traditional exports inhibit the capacity for innovation and reduce land ownership to a mechanism for extracting a surplus from an economy with a low level of production rather than serving as a basis for organizing agricultural production; inelasticity of market; import-substitution limited to nondurable goods; the braking effect of the gold standard and the structure of international finance because of the high volume of foreign exchange reserve required; the difficulties in developing technological competence as new industries are simply local subsidiaries of international groups, mainly North American; and a new and complex form of dependence on the outside world through the growing use of international credit agencies such as the World Bank as financial intermediaries for national governments. The variations in national situations are drawn from the cases of Argentina, Brazil, Chile, Colombia, and Mexico and after 1950, Venezuela and Peru.

One does not have to accept the theoretical validity of Furtado's position, but one would be ill advised to attempt an understanding of what is taking place in most Latin American nations today without a comprehension of his vision of the "wide process of social change as seen in the context of historical reality." It is more fully available in this book than in any of his earlier essays.

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Coral Gables

FREDERICK C. TURNER, *Catholicism and Political Development in Latin America*. Chapel Hill: University of North Carolina Press. 1971. Pp. xv, 272. \$8.75.

Since Fidel Castro came to power, the social and economic crises that afflict most of Latin America have aroused expressions of concern among some of the region's lay and clerical Catholic leaders. Professor Turner's book examines and vigorously applauds these newly

emerging patterns of Catholic thought. According to Turner, who has assembled an impressive array of interviews, pastoral messages, and periodical articles to buttress his case, increasingly numerous and influential elements within the laity and the clergy have reoriented their goals and are calling for an activist, socially concerned Church that encourages participation in reformist political movements. Although he admits that powerful traditionalist elements oppose the reformers, he hypothesizes that already between one-fifth and one-third of the clergy is "progressive," and he is cautiously optimistic that reform trends will continue. Eduardo Frei, Christian Democratic president of Chile between 1964 and 1970, becomes for Turner a model of what concerned Catholic laymen can achieve. United States foreign policy, we are advised, ought discreetly to throw strong support to progressive Catholic political groups that achieve power.

The book is packed with worthwhile anecdotes and interesting information, much of it new. Career sketches of reform leaders like Father Camilo Torres and Archbishop Hélder Câmara, along with illuminating analyses of the Church's accommodations with authoritarian political leaders ranging from Stroessner in Paraguay to Castro in Cuba, make for lively reading. The book may achieve one of the author's goals, to stimulate public attention in the United States to the changes taking place within Latin American Catholicism. But Turner, in my judgment, does not prove his case that the reformers represent the wave of the future. The book unfortunately avoids studying the exercise of power within the hierarchical and bureaucratized Latin Churches. The seemingly endless quotes from progressive Catholic intellectuals that the author employs hardly add up to a convincing argument that the hierarchy in most countries is willing to make basic policy changes. Turner apparently hopes that an aroused laity will eventually force the leadership to change course, but the book's lack of empirical evidence as well as its strong reliance on the probably unique Chilean case weaken the argument. As Turner himself admits, we need much empirical research on Latin American Catholicism.

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HUGH THOMAS. *Cuba: The Pursuit of Freedom*. New York: Harper and Row. 1971. Pp. xxiv, 1696. \$20.00.

This is a remarkable book—all 1,700 pages, 116 chapters, and 14 appendixes, 11 “books,” an epilogue, and five and a half pounds of it. It is not simply that it is monumental, virtually encyclopedic; it can also be fairly described as balanced, thoroughly researched, and insightful. For the narrative of the decade from 1952 to 1962 it might be considered definitive.

The author characterizes his own study: “Half of it, plainly, is history; but in the second half I enter upon contemporary politics, and by the time the Revolution of 1959 is reached I am in a no-man’s-land between history, politics, sociology and journalism.” In a more Orthodox way Thomas has set himself the task of presenting the two centuries between the British capture of Havana in 1762 and the missile crisis of 1962. The post-1962 years are treated more summarily in the epilogue, and narrative is secondary to analysis.

The initial date of 1762 is more defensible than the terminal one of 1962. Prior to 1762 Cuba had been largely stagnant; after the British withdrawal in 1763 as part of the general settlements of that year, conditions in Cuba were never really the same, even during the next century of seemingly solid Spanish control. But to end the study, in effect, with the resolution of the missile crisis is unfortunate. Granted that October 1962 was a crisis point, the author’s sources for the next half dozen years must have been virtually as good as for the preceding half dozen and, recognizing the author’s wariness of trying to write “recent history,” it still would have been most useful to have his same detailed treatment of the rest of the 1960s.

Professor Thomas (of the University of Reading) has consulted an amazingly wide variety of sources; much of the account of the 1960s is drawn from personal contacts made on a number of trips to Cuba. In very skillful fashion he weaves together, on an almost day-to-day basis for critical periods, the tangled skein of developments.

Thomas rigorously excludes incidental value judgments, so often introduced even unconsciously by writers on controversial topics in

their choice of adjectives, their introduction of casual laudatory or critical phrases, and in other ways. In certain chapters or parts of chapters, of course, he designedly enters into analysis and evaluation and in them he does not hesitate to speak his mind. Perhaps one of the severest tests of the objectivity of any writer on contemporary Cuba is how he deals with Fidel Castro. In general Thomas goes right down the middle, viewing the *líder máximo* neither through the rose-tinted glasses usually worn by Herbert Matthews nor with the passionate and pathetic hostility of, for example, Manuel Urrutia. His stance as an Englishman adds to his objectivity.

For the most part the study is oriented toward political developments, but occasional chapters digress to deal with social organization (in both a broad and a narrow sense), economic problems and activities, the cultural and literary scene, and other matters. The appendixes introduce a wide variety of materials of genealogical, ethnological, statistical, anecdotal, and other sorts. Useful capsule biographical information about some of the lesser actors in the marathon unfolding is occasionally included in the meticulous footnoting.

In not too obtrusive fashion Thomas deals from time to time with the problem of his volume subtitle, the Cuban pursuit of freedom. In his concluding pages, for example, he says: “Yet the obsession with freedom creates its own bondage, and is there not doubt whether in any real sense even Castro is a free man? He imposes his personality on Cuba but, like all Cuban rulers, he is at the mercy of the sugar markets as of the twenty-year relative stagnation in the Cuban economy which he has not arrested. In part, too, he is the creation of the dreams of Cubans for a revolutionary leader of epic stature, just as he is the articulate expression of a nation whose ‘authentic qualities’ include what is usually known as ‘gaiety.’ . . . The long shadows of past habits stretch across the most radical reforms, either blacking them out or giving them quite different colours.”

This study goes far toward clarifying the colors in the confused kaleidoscope that is contemporary Cuba.

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Santa Barbara

FRANKLIN W. KNIGHT. *Slave Society in Cuba during the Nineteenth Century*. Madison: University of Wisconsin Press. 1970. Pp. xxi, 228. \$10.00.

The hypothesis that slavery in the Iberian colonies was more humane than that of the Anglo-Saxon colonies and countries was given scholarly repute by Frank Tannenbaum in his book, *Slave and Citizen: The Negro in the Americas* (1946). Later writers developed the Tannenbaum thesis, including Herbert S. Klein who produced a book, *Slavery in the Americas: A Comparative Study of Virginia and Cuba* (1967). Contrasted with the arbitrary power of slaveowners in Virginia, Klein contends that Cuban master-slave relations were tempered by the intervention of priests and public officials. Moreover, he points to urbanization and agrarian diversification as factors influencing the lighter work load of slaves in Cuba compared with those in Virginia.

Franklin Knight has marshaled literary and statistical evidence that clearly modifies the Tannenbaum-Klein thesis as it pertains to Cuba in the nineteenth century. He finds that a high proportion of the slaves were rural and attached to sugar plantations, that a new slave-owning class emerged that was hostile toward the humanitarian attitude of Church and state, that the practice of manumission declined as the demand for labor increased, and that slave codes were not enforced. Overwork and underfeeding made the slaves susceptible to disease and premature death. Most damning to the myth of humane treatment was the net rate of slave population decrease, which ranged from three to five per cent per annum in the middle decades of the nineteenth century.

Sugar and slavery have had a centuries-long relationship whereby great wealth has accrued to Europeans and North Americans who combined their skills and capital with imported labor on tropical plantations. On successive frontiers the factories and cane fields have used up the labor of millions of Africans who were often cheaper to buy from the slavers than to breed by means of humane treatment by their masters. As a West Indian himself, Franklin Knight rightly contends that comparative study of slave systems should consider both the nature of the slave society and the fact of the sugar revolution. Moreover, he faults earlier

writers whose comparative studies of slavery neglect equivalent stages of economic and social growth and exaggerate the influence of metropolitan institutional differences.

I find a few faults in the book, though they are minor in the light of the important contribution it makes. Plantation agriculture in Cuba was not distinctive with respect to technical innovation. While it is true that Cuban planters invested in railroads and steam factories, planters elsewhere in the Caribbean had earlier been technologically progressive in adopting windmills, watermills, and irrigation systems. Since the mechanization of sugar processing coexisted with hand labor in cane cultivation and harvesting, it is difficult to follow Knight in his contention that the demise of slavery was caused in large part by the revolution in the manufacture of sugar. More important to the demise was the substitution of cheap indentured laborers from China and India.

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D. A. BRADING. *Miners and Merchants in Bourbon Mexico, 1763-1810*. (Cambridge Latin American Studies, Number 10.) New York: Cambridge University Press. 1971. Pp. xvii, 382. \$16.50.

Armed with the methodology of the social scientist, many young scholars of late have revisited colonial Hispanic America with profit. Dr. Brading is one of them. His book on Bourbon Mexico is an exhaustive effort based almost entirely upon Spanish and Mexican documentation.

The author's purpose was to define this crucial period in both human and economic terms. As his vehicle he chose three autonomous studies: first, "Revolution in Government," in which he presents the background and evaluates such factors as the visitation of José de Gálvez (1765-71), the expulsion of the Jesuits (1767), and the Bourbon reforms; second, "Miners and Merchants," in which he analyzes their origins, activities, and institutions; and third, a detailed examination of the prosperous Guanajuato mines and community. He expertly uses notary records, petitions, genealogy, production figures, and numerous tables and graphs, many of which are reproduced and

discussed in the text. His conclusions are sound and convincing.

Guanajuato's remarkable productivity resulted from the Bourbon reforms and incentives, the availability of merchant capital in the late 1780s, the miners guild (1783), and the investment skills of the entrepreneurs. Dr. Brading's analysis of Mexican society is another rewarding feature of this work as he traces the emergence of a new, aggressive elite consisting of immigrants from northern Spain. Favored by the government and relatives in Mexico, these moral and hard-working Spaniards soon reached the top of the economic structure and consolidated their position in society by marriages to Creole women, by purchases of haciendas that they entailed, and by securing titles of nobility. Unable to compete, Creole men suffered accordingly. The situation worsened in the days before independence.

The Guanajuato study is unquestionably an outstanding contribution. The earlier sections, however, are merely exploratory. By opting to concentrate on structures rather than the operational aspects of reform, the author limited the validity of his conclusions. Moreover, the emphasis upon central Mexico and the neglect of outlying zones—the Consulados of Guadalajara and Vera Cruz, for example—ignored a comparative element in what was also Bourbon Mexico. Considering the importance of contraband elsewhere in Spanish America, it is likewise difficult to believe that it played no substantial role in New Spain. Let us hope that Dr. Brading will continue to develop the first two themes so that they may reach the level of his excellent work on the mining community of Guanajuato.

MARIO RODRÍGUEZ

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JAN BAZANT. *Los bienes de la Iglesia en México (1856-1875): Aspectos económicos y sociales de la Revolución liberal*. (Centro de Estudios Históricos, New Series, Number 13.) [México, D. F.:] Colegio de México. 1971. Pp. xiii, 364.

JAN BAZANT. *Alienation of Church Wealth in Mexico: Social and Economic Aspects of the Liberal Revolution, 1856-1875*. Edited and translated by MICHAEL P. COSTELOE. (Cambridge Latin American Studies, Number 11.) New York: Cambridge University Press. 1971. Pp. xvi, 332. \$17.50.

Serious work on the material resources of the Catholic Church in nineteenth-century Mexico is now well under way. The present study is so far the most ambitious and adds significantly to the research of Michael Costeloe (Bazant's English translator), Charles Berry, and Robert Knowlton. Bazant has not, however, written the definitive study on Church wealth, as he readily admits. His investigation did not encompass the whole of Mexico, and he did not utilize all the major resources available, for example, the diocesan archives in the areas covered. Furthermore his economic analysis is open to question because he fails to establish a solid basis for computing the market value of property. Finally, his acceptance of certain statistics—for example, that there were 3,066 houses in Puebla for a population of 70,000 and 4,000 in Mexico City for a population of 200,000—casts doubt upon the conclusion that the Church controlled about half the real estate in these two cities. Some questions, of course, may never be answered with any precision either because of the destruction of relevant records or because of inaccurate or incomplete data from the beginning.

Bazant has settled in a general way some of the arguments raised in the bitter polemics that have raged for over a century between clericals and anticlericals. First, his best estimate of the value of all Church real estate and mortgages is between seventy and eighty million pesos (in contemporary not present-day currency). This is a far cry from the oft-repeated claim that the Church owned one-half of the wealth in Mexico, even if we accept the extraordinarily low estimate of 340 million pesos as the value of all real estate in Mexico in 1870. On the other hand the official method of computing the value of Church holdings by capitalizing the rents and the interest payments on mortgages at five per cent may well undervalue the properties. Whatever their value Church holdings were divided approximately five-sixths in urban property and only one-sixth in rural.

Second, the Ley Lerdo of 1856, which required the Church to sell its real estate but permitted it to hold the mortgages, and the expropriation law of 1859 had their roots in Mexican history both in the colonial and the national periods. In a sense the laws on Church

property of the Reforma period are a culmination of historical precedents, not a sharp break with the past. Conservative as well as Liberal governments sought to acquire Church property to pay their debts in the years prior to the nationalization in 1859. The primary difference was that the Church parted cautiously but willingly with its property to the Conservatives and most unwillingly to the Liberals; but part with it she did. Ironically Conservatives (as well as Liberals) acquired Church properties during the 1850s and 1860s, became committed to the changes, and refused to consider returning the properties to the Church under any circumstance.

Finally, while vast amounts of property fell into relatively few hands (businessmen, professionals, government officials, and landowners), a substantial number of modest-income families did gain possession of their residences. Rural property largely fell to the already wealthy, and while foreigners got possession of a substantial amount of urban property they obtained almost no haciendas. Certainly a rural middle class was not created as the Liberals hoped it would be. The national treasury profited only marginally from the expropriations, largely because the government of Juárez was hard pressed for funds to fight first a civil war against domestic enemies and then against the French invaders. Under these difficult circumstances, property was at times sold off for less than three per cent of its assessed value.

The English version is a faithful translation of the original and basically parallels it except for some minor alterations. Appendix 1 in the original is included in the introduction of the English translation and four other appendixes are omitted since they have already appeared in English. There are also a fair number of changes in the bibliography and some rather substantial alterations in the English preface, which, however, is signed by Bazant.

This is a major work. It is not easy to read, especially for laymen unversed in banking, finance, and mortgage contracts. Its organization and style leave much to be desired, but its findings are often fascinating and its conclusions provocative.

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PAUL FRIEDRICH. *Agrarian Revolt in a Mexican Village*. (Anthropology of Modern Societies Series.) Englewood Cliffs, N. J.: Prentice-Hall. 1970. Pp. xvi, 158. Cloth \$5.95, paper \$3.25.

ROGER D. HANSEN. *The Politics of Mexican Development*. Baltimore: Johns Hopkins Press. 1971. Pp. xiii, 267. \$11.00.

CLARK W. REYNOLDS. *The Mexican Economy: Twentieth-Century Structure and Growth*. (Publication of the Economic Growth Center, Yale University.) New Haven: Yale University Press. 1970. Pp. xxiv, 468. \$13.50.

The three books under review admirably demonstrate the improvement of social science research on Latin American topics that took place during the 1960s. The professional backgrounds of the authors are varied: Friedrich is an anthropologist, Hansen is (presumably) a political scientist, and Reynolds is an economist. Yet, all contribute in ways appropriate to their disciplines to a more sophisticated understanding of twentieth-century Mexican history.

Professor Friedrich's work is surely innovative among anthropological studies of Mexico. Departing from the tradition of community studies (engendered decades ago by the Chicago school of urban sociology) he explores the making of a revolutionary, Primo Tapia. In the process he calls attention to the peculiar importance of living in the United States for the returned Mexican immigrant and the exigencies of race and class conflict in the making of a revolution.

Primo Tapia led the agrarian revolt in his native village in the 1920s and achieved the goal of returning a Spanish-owned hacienda to community control in 1926—not long before his assassination by government troops. Two aspects of his personal experience were of major importance in his preparation for revolutionary leadership: his attitude toward authority and his experience with anarcho-syndicalism and the Wobblies in the U.S. As Friedrich puts it, "Primo had strong hatreds. The positive struggle for the fertile black soil was complemented by unmitigated hostility against the Catholic clergy, the Spanish landlords, and the entire network of exploiters, that is, against all who were not impoverished peasants, industrial workers, or their representatives. . . . His hostility toward authority and his learning of anarchist theory partly meshed with



the vigorous local autonomy for which the Tarascans are renowned and for which the Naranjeños are particularly conspicuous" (pp. 74-75). Tapia imbibed anarcho-syndicalism among the Wobblies of California and in 1920 even led a strike against a beet sugar refinery in the state of Nebraska. His training for revolution thus came in the U.S. During the agrarian revolt itself he succeeded in uniting poor mestizos in the neighborhood to the cause of his fellow Tarascans. He was then instrumental in turning a race conflict between mestizos and Indians into a class conflict between the large landholders and the landless, Indian or white. These aspects alone of Friedrich's study should encourage historians of the United States to examine his findings. This book should prove useful to those who seek to learn more (and to convey to their students) a better sense of the relationship between the U.S. and Mexicans.

More topical perhaps is Hansen's study of policy making for economic development. It seems an excellent summary and critique of the extant literature on the subject, and it correctly emphasizes the central problem of Mexico's economic success story: who gets (or got) what? One of his chapters is appropriately titled "The PRI and Mexican Politics: La Cosa Nuestra." Some readers will be disappointed by Hansen's reliance on already well-known secondary sources and may well ask what, if anything, is new in his book. However, they also serve who only synthesize.

Perhaps the more striking synthesis (as well as much more) is that achieved by Reynolds in his investigation of Mexican economic history in the twentieth century. It is difficult to imagine a major development problem that this author has not treated in his work. Thus one attaches considerable weight to his conclusion that "it is difficult to see how an alternative mixture of public policies could have increased the rate of growth or substantially improved the level of income of all sectors of the population since 1940, and it is easy to imagine how more extreme measures might have retarded the growth rate" (p. 310). The agrarian revolution of the pre-1940 period and, more important, the creation of the ethos of revolution were a necessary prelude to the rapid growth of urban activities and the highly efficient plantation agriculture of the North and Northwest.

Reynolds suggests that this "attitudinal" factor may explain up to forty-five per cent of Mexican economic growth in the years between 1925 and 1960 (p. 56). Agrarian reform returned some land to the tiller, moderated the rate of cityward migration, and turned the attention of the poor away from the growing wealth of the rich. The extraordinary economic growth of the past quarter century (exceeded only by Japan and some European countries) may not have been possible had the fruits of growth been more equally distributed.

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ISAAC S. and SUZANNE A. EMMANUEL. *History of the Jews of the Netherlands Antilles*. Volume 1, *History*; Volume 2, *Appendices*. Cincinnati: American Jewish Archives; distrib. by Ktav Publishing House, New York. 1970. Pp. 533; 539-1165. \$35.00 the set.

Curaçao, captured by Holland in 1634, became the New World haven for Sephardic Jews. Jewish merchants aided the island's development, achieving prominence in the eighteenth century when their 280 families comprised half of the white population. The "Portuguese Jewish Nation" of Curaçao functioned as a separate estate controlled by wealthy congregation trustees. Since their decisions were enforceable by civil authorities, trustee authoritarianism often aggravated intra-Jewish tensions and, after equal civil status had been obtained, finally resulted in the schism of 1864. Since then Curaçao Jewry has declined in importance and religious zeal. Such is the focus of *History of the Jews of the Netherlands Antilles*, which, belying its title, is actually a study of Curaçao's Sephardic Congregation Mikveh Israel, the oldest in the Western Hemisphere.

Dr. Isaac S. Emmanuel served as rabbi in Curaçao where he wrote *Precious Stones of the Jews of Curaçao* (1957), a masterful study of twenty-five hundred tombstones patterned after his genealogical research in Salonika. His latest handsomely illustrated effort is based on exhaustive archival research in Curaçao and Holland that provides an insight into the problems faced by small Jewish enclaves everywhere. Its wealth of detail would do justice to many larger Jewish communities. Some of the appendixes—marriages and Curaçao expatriates, for

example—furnish extremely useful data. Yet Emmanuel's work suffers in historical craftsmanship, probably because he attempts to narrate general as well as institutional history in the setting of a reference work. His inclusive approach to history emphasizes documentation over interpretation, cases rather than trends. The result is uneven and haphazard, with disjointed chapter development and blurred criteria in the selection of materials for the two separate volumes. Critical relationships, such as that of climate to occupation or invasion to foreign policy, are drawn sketchily if at all. Also missing are useful maps, population charts, and coinage tables. The index (4,400 items) concentrates on family names, while the bibliography unfortunately foregoes secondary sources. Emmanuel mistakenly assumes his reader's knowledge of Curaçao and Judaism. Several introductory chapters to provide that kind of background would have greatly enhanced the significance and clarity of this history.

Dr. Emmanuel's book is uncommonly candid for ethnic history. The various altercations he describes furnish the most readable sections. However, contradictions concerning the importance of the Jews to Curaçao or their relationships with Gentiles do appear. Furthermore the author is not entirely forthright with regard to mulattoes descended from Jews, who did have an advantaged position. Censorship by the trustees may have been responsible for some omissions, particularly where the 1964 merger of the reform and traditional communities are concerned. Emmanuel was visibly saddened when "the elegant, lucid Sephardic rite" was replaced by "irrelevant Reconstructionism," a substitution that, he maintains, will not stem the tide of Jewish assimilation in Curaçao.

BERNARD D. ANSEL  
State University College,  
Buffalo

GERMÁN COLMENARES. *La provincia de Tunja en el Nuevo Reino de Granada: Ensayo de historia social (1539-1800)*. Appendices transcribed by MARÍA CRISTINA MURILLO. Bogotá: Universidad de los Andes, Facultad de Artes y Ciencias, Departamento de Historia. 1970. Pp. 283.

This is one of a series of archivally based mon-

ographs on colonial New Granada now being produced by a small number of able young Colombian historians of modern orientation. The emergence of this new generation represents an important development in Colombian historiography. Previous Colombian scholars had toiled through the archives, but the work now being done is more fruitfully informed by the concerns, methods, and interpretive insights of the most sophisticated and innovative social historians now working on colonial Spanish America.

Colmenares's monograph on Tunja is a preliminary exploration, more of a report from the archives than a finished work. It has interesting ideas throughout but lacks an integrating structure. Some material that may be useful to future historians is not placed at the service of an interpretive point. The work is not pulled together even in a formal sense: the chapters lack unifying introductory and concluding paragraphs; nor is there a general conclusion at the end. This is in contrast with Colmenares's sprightly book on mid-nineteenth century politics, *Partidos políticos y clases sociales* (1969), a work more in the traditional literary mode but one of structure and confidently asserted interpretation. While less satisfying esthetically, Colmenares's current tentativeness does reflect a greater sobriety in confronting the complexities of social analysis in a field in which data is still very sparse.

The Tunja monograph discusses sketchily aspects of Indian social, economic, and religious organization before and immediately after the Spanish Conquest. Later sections provide a more substantial treatment of Indian demographic decline and the process of *mestizaje*, the evolution of Indian tribute and other forms of colonial labor exploitation, and the constriction of Indian community lands. On each of these subjects Colmenares's findings correspond closely to those of his acknowledged mentors working on other parts of Spanish America—most notably those of Woodrow W. Borah, Charles Gibson, Rolando Mellafe, and James Lockhart. But while Colmenares incorporates perceptions from the better works on colonial society, he also displays his own very active historical imagination. He follows Lockhart, for example, in emphasizing the urban centeredness of the European population in

the sixteenth century. To this he adds the point that *mestizaje* is primarily an urban phenomenon, with mestizos forced out of the urban centers by Creole exclusivism, thereby coming to dominate the rural world of the Indian.

In the tricky field of historical demography, Colmenares is suitably cautious. He is sensibly skeptical of the reliability of *visita* figures, and he entirely avoids the problem of the size of the pre-Conquest population, considering it insoluble. Following the Colombian examples of Jaime Jaramillo Uribe and Juan Friede, he discriminates among tribute data from different regions and time periods. In his analysis of demographic decline, Colmenares emphasizes the role of Spanish labor requirements in limiting sexual contacts among the Indian population. He follows Mellafe in noting a decreasing proportion of tributaries in the Indian population, which he sees as indicating a disintegration of the family as adult males migrated to escape tribute obligations.

While this monograph does not tie all of its material into neat interpretive bundles, it is a useful and informative effort in an area in which very little research has been done.

FRANK R. SAFFORD  
Northwestern University

E. BRADFORD BURNS. *A History of Brazil*. (Institute of Latin American Studies, Columbia University.) New York: Columbia University Press. 1970. Pp. xii, 449. \$11.95.

This substantial text on Brazilian history joins two previous works by the author, *A Documentary History of Brazil* (1966) and *Perspectives on Brazilian History* (1967), to form a teacher's trilogy on that insufficiently studied giant of the Americas. In his introductory remarks Professor Burns promises a new approach to Brazil by means of thematic studies emphasizing the role of the masses. Readers will discover, however, that *A History of Brazil* provides them instead with a traditional political chronology into which intellectual, economic, and social threads frequently and effectively are woven. As a result this work will serve admirably as a useful reference work on Brazilian history or as a textbook for Brazilian history courses; it is

unlikely to engage or arouse the casual reader or the uncommitted student.

Unfortunately the first chapter lacks the occasional drama and skillful organization the author brings to subsequent sections. An effective sketch of Indian and European backgrounds makes up somewhat for an uninspired geographical description, but as a whole this chapter seems lost under a mass of detail. Such an impression only receives reinforcement from two unimaginative and amateurish maps on states and rivers that reflect little credit on Columbia University Press designers.

Professor Burns displays his considerable knowledge of Brazilian history in a far more attractive and convincing fashion in the remaining six chapters. The colonial experience emerges against the backdrop of world events and shows the mutually self-reinforcing nature of Brazilian and Portuguese development. The independence section starts with the ideas of the Enlightenment, although too soon it turns to political narrative. The description of Brazil's nineteenth-century transformation reaches high points with the detailed explanation of the impact of coffee, the communication-transportation revolution, the parallel intellectual changes, and the abolition movement. A chapter on the new Brazil, 1888-1922, examines the coffee boom of the early twentieth century, the rise of the cities, and the myriad and often confusing political events of the Old Republic. Professor Burns then advances the thesis of a conscious restructuring of society, emphasized by the 1922 Modernist movement and the rise of Getulio Vargas. The confusing events since 1945 are neatly summarized in a final chapter on reform, radicalization, and reaction.

In each chapter Professor Burns attempts to link literary and artistic movements to political and economic trends. He clearly attributes key roles to intellectual movements and changes in attitudes. Two major periods, the independence of Brazil and the Vargas era, are seen as conscious efforts involving not only the leaders but also sizable sectors of the population. Such deliberate redressing of the balance toward intellectual history richly deserves commendation and perhaps emulation by other students of Latin American history who tend to lose sight of intellectual currents in their pursuit of political or economic forces.

The book's supporting aids do not seem to reach the level achieved by the text. The first two maps, as mentioned above, are deficient; the third, regions of Brazil, shows current imbalances in population and national income in an unimaginative format. Scattered throughout the text are several well-constructed tables, and a cluster of twenty-seven photographs near the end of the book provides a visual glimpse into the variety and richness of Brazil's past. Less useful are the appendixes containing a confusing list of chiefs of state, excerpts from the five Institutional Acts of the post-1964 governments, selections from the 1967 Constitution, and a list of significant political dates. The book closes with a clearly organized list of further readings emphasizing available literature in English.

*A History of Brazil* deserves a place on the shelf of any student of Latin American history. In this useful reference work, Professor Burns has collected and organized a wealth of data, marshaled themes in chronological format, and dressed the narrative in an attractive style of writing.

JAMES R. SCOBIE  
Indiana University

GEORGE WOODCOCK. *Henry Walter Bates: Naturalist of the Amazons*. (Great Travellers.) New York: Barnes and Noble. 1969. Pp. 269. \$5.50.

Henry Walter Bates discovered eight thousand new species, mostly insects, during eleven years in Brazil (1848-59). That should qualify him for historical mention, especially since establishment scientists of the British Museum did not believe him and had to be proven wrong. Lacking a university degree he could never land a job that would employ his entomological knowledge, and he ended his career as assistant secretary of the Royal Geographical Society.

He had long since won public approval for his superb account of the Amazon experience, and it is this unsponsored trip, which Bates began at age twenty-three in the company of Alfred Russel Wallace, that forms the core of this work. Woodcock's study is part of the Great Travellers series designed to revive popular interest in "important but neglected wanderers." That Woodcock does, but for four-fifths of the book one inevitably gets little more than rehashed Bates, albeit nicely done,

interspersed fortunately with numerous quotes in the delightful prose of Bates himself. Woodcock tries to interweave other material, but the pickings are scarce. Let the reader turn to Woodcock for introduction and conclusion, but for the trip itself there is no substitute for Bates's own account.

Woodcock sees Bates as a potential peer of Wallace and Darwin in the development of evolutionary theory. Bates let his opportunity slip by because of his compulsion to seek more data. His career never again reached the peak of his Brazilian years, and we should perhaps be grateful to Mr. Woodcock for reminding us to relive that peak through the words of Bates himself.

ARTHUR R. STEELE  
University of Toledo

JOSEPH L. LOVE. *Rio Grande do Sul and Brazilian Regionalism, 1882-1930*. Stanford: Stanford University Press. 1971. Pp. xv, 311. \$10.00.

Mr. Love presents an excellent study that is much broader in scope than the title suggests. The basic theme is the pivotal role played by the Gauchos in the decline of the Old Republic. The participation of Rio Grande do Sul in national politics is divided into four phases: dependence on military presidents, Deodoro and Floriano, 1889-94; autonomy and relative isolation under Governor Castilhos following the civil war, 1895-1903; gradual emergence as a major political force under Pinheiro Machado and Borges de Medeiros, 1904-08; and full-scale participation in national politics, 1909-30.

The era of the Old Republic (chapters two through ten) is the heart of the work. In it the author notes and provides numerous examples but does not always stress the sellout of democracy by the republicans, the failure of the republican parties to transfer power to a younger generation on the national level, the ability of São Paulo and Minas Gerais to prevail in the presidential elections if they act in concert and possibly face a revolution every four years (a revolution that the other states could only hope to win with the support of the armed forces), and the will to power of the military in conjunction with the ambitions and aims of the leaders of Rio Grande do Sul—ultimately those of the generation of 1907, as the author aptly dubs Getúlio Vargas and his contemporaries.

The work is solidly based on the personal archives of many of the leading figures. But it is curious to note that Borges de Medeiros, who provides the thread for most of the book, never emerges as a sharply defined character and is summarily dismissed in a sentence in the last chapter. This chapter, number twelve, along with the introductory chapter are the weakest parts of the work. This book, in spite of the author's occasionally guarded style, is a capital contribution to our understanding of regionalism and the Old Republic.

LEWIS A. TAMBS

*Arizona State University*

RONALD M. SCHNEIDER. *The Political System of Brazil: Emergence of a "Modernizing" Authoritarian Regime, 1964-1970*. (Institute of Latin American Studies, Columbia University.) New York: Columbia University Press. 1971. Pp. xviii, 431. \$13.50.

The army leaders who took over Brazil in April 1964 hoped that the "exceptional powers" necessary for fighting "subversion, corruption, and inflation" would be few and short-lived. However, as Professor Ronald H. Schneider shows, in the years following the 1964 takeover the military leaders responded to challenges of the antimilitary opposition by abandoning all limits on dictatorship. The abandonment was done in a "legalistic" flurry of Institutional Acts, "complementary acts," and decree laws. Rules were forever being changed, often under pressure reportedly exerted by the antidemocratic "hard line" wing in the military.

As a background for this story of crises and cassations (cancellations of the political rights of individuals), Schneider supplies "a selective summary" of his forthcoming book about Brazil, 1889-1964—a book that he has delayed publishing in favor of his present volume because, as he explains, already Thomas E. Skidmore has contributed greatly and Vladimir Reisky de Dubnic to a lesser extent to our understanding of the events leading up to 1964, whereas significant analytical literature on the post-1964 regime has been lacking.

Five chapters, consisting of 222 pages, make up the "chronological narrative," 1964-70, or "body" of Schneider's new book. This meticulous, accurate, and well-organized account of political and military matters is a tremendous

achievement—splendidly written and so complete that it is indispensable for anyone interested in learning the details of what happened. In guiding the reader from the "manipulated democracy" of the early part of Castelo Branco's regime to the "descent into dictatorship" of the last part of Costa e Silva's regime, and on through the first year of Garrastazú Médici's regime, Schneider supplies copious comments of Brazilian observers and a useful section on military education.

The book is stimulating. While political scientists can argue whether or not Schneider's final chapter is a valuable contribution to systematic political science, other scholars have the opportunity of agreeing or disagreeing with the author's pessimism. At the outset Schneider declares that "Brazil is, among the nations of the world, an adolescent giant in severe trouble." Later, pondering dicta formulated by Samuel P. Huntington, a fellow political scientist, Schneider concludes that the Médici administration fails to appreciate "the need" of developing peasant support and political institutions. Apparently writing late in December 1970, Schneider finds that "from December, 1968, on, the decay of military unity has been painfully evident," and that Médici's "prospects for serving out his full presidential term would seem to be no better than the average for the Brazilian Republic—less than a fifty-fifty proposition."

JOHN W. F. DULLES  
*University of Texas,  
Austin*

J. R. FISHER. *Government and Society in Colonial Peru: The Intendant System, 1784-1814*. (University of London Historical Studies, Number 29.) [London:] University of London, Athlone Press; distrib. by Oxford University Press, New York. 1970. Pp. ix, 289. \$12.00.

A comprehensive evaluation of the functioning of the intendency in the Spanish Empire is long overdue. The neglect is difficult to explain, since the administrative innovation represents the paramount political reform of the Bourbons. The present volume by a lecturer at the University of Liverpool is a solid contribution to our knowledge of the role of this official in the viceroyalty of Peru.

Preceding its Mexican counterpart, the Peru-



vian intendancy marked another stage in the realization of the twin Bourbon goals of administrative centralization and absolutism. To the second visitor-general Jorge Escobedo was assigned the arduous task of introducing the new system, which meant in large measure overcoming the stubborn resistance of the bureaucratic establishment headed by the viceroy. There has been perhaps no more vigorous individual than Escobedo in the last fifty years of the kingdom of Peru. His career emphasizes once again the interrelationship of institutional evolution and personality. Did the adoption of the intendancy revitalize colonial government? The author's answer is a carefully qualified affirmative. The intendants managed to increase revenue for at least a decade through meticulous collection of taxes and elimination of fraud and corruption. Public administration was improved by the construction of roads, bridges, and government buildings and through the regulation of food supplies. While commercial activities were stimulated, it proved impossible to increase agricultural production. Probably the intendancy found its greatest success in the realm of municipal affairs. Moribund *cabildos* came to life by appointment of new councilors, expansion of revenues, and a program of public works. Did this mean royal oppression? The *cabildos* thought so, at any rate. That any significant alleviation of the exploitation of the Indians—dramatically disclosed earlier in the rebellion of Tupac Amaru—took place is seriously debatable.

For the most part the author's conclusions confirm the present view of the place of the intendancy in the colonial administrative system, but studies of a similar nature will be necessary to complete the picture. Generally speaking his treatment follows that of Professor Lynch, whose excellent account of the intendancy in the viceroyalty of Buenos Aires initiated scholarly investigation of this feature of government. The abundance of archival sources consulted in Spain and Peru is commendable. Undoubtedly this study will be a valuable supplement to the growing institutional literature of the Spanish Empire.

J. P. MOORE  
Louisiana State University

JAMES M. MALLOY. *Bolivia: The Uncompleted Revolution*. [Pittsburgh:] University of Pittsburgh Press. 1970. Pp. x, 396. \$11.95.

JAMES M. MALLOY and RICHARD S. THORN, editors. *Beyond the Revolution: Bolivia since 1952*. [Pittsburgh:] University of Pittsburgh Press. 1971. Pp. xvii, 402. \$11.95.

Writing about unfolding social and economic revolutions poses occupational hazards because research may become outdated or need revising immediately after publication. In the Bolivian revolution, launched in 1952 and controlled or wracked by military factions since 1964, recent events cast doubt upon some of the assertions presented here. The successful revolt led by Colonel Hugo Banzer in 1971 against the radical regime of General Juan José Torres caused the most violent resistance since the *Movimiento Nacionalista Revolucionario* (MNR) seized power in 1952. But this party, which began the transformation of Bolivian society, joined forces with its fascist opponent for three decades, the *Falange Socialista Boliviana*, to back the Banzer putsch. Thus, Víctor Paz Estenssoro, major architect of the MNR who was beginning his third term in 1964, became a pathetic figure hungry for power at any price. Where is the revolution now?

The scholars who have tended to apologize for the totalitarian origins of the MNR should re-examine the historical record. Cole Blasier states that "many of these charges [of Nazi influence in 1941] were misleading, exaggerated, or false." Yet the newspaper *La Calle*, published by the MNR, conducted a vicious anti-Semitic campaign during debate on the 1942 immigration bill, in which deputies Paz and Siles both voted for the exclusion of Jews, Negroes, and Asians from Bolivia. James M. Malloy also glosses over the totalitarian aspects of the regime of Major Gualberto Villarroel (1943-46), which the MNR supported. For example, MNR journalist Roberto Hinojosa, among those whose corpses were strung up from lamp posts in 1946, had published pamphlets glorifying the "life and passion" of Adolf Hitler.

In his book Malloy writes that the depression of the early 1930s widened the chasms in Bolivian life later exposed by the disastrous Chaco defeat. He thinks the 1952 revolt oc-

curred partly because economic stagnation had dimmed the expectations of the younger and later more deeply disillusioned men of the middle sector, both civil and military, who thus became "reluctant revolutionaries." (Richard Patch earlier called Bolivia's experience a "restrained" revolution.) Malloy believes that not only was agrarian reform thrust upon these leaders, but also nationalization of the Big Three tin mines. This is inaccurate because only minor MNR figures resisted nationalization as the major goal of the MNR since Carlos Montenegro's *Nacionalismo y coloniaje* won the national literary prize in 1943.

Malloy more clearly sees the pragmatism of central MNR leaders who were forced to meet haphazardly demands of both the mine workers of the Left led by Juan Lechín (elected president of the presidium of the Popular Assembly in 1971) and the job-seeking, increasingly alienated men of the middle sector who also fought for the movement. The enfranchised Indian masses became an uncertain base of support, eventually requiring the fatal reconstruction of the Bolivian military establishment by the United States.

Political backlash exploited during the Torres regime obtained more in aid agreements from the Soviet bloc in ten months than Bolivia received from the United States in twelve years. Both Blasier and Richard S. Thorn predicted the staggering price paid in August 1971 by Bolivia for this aid. Some Bolivian writers feared that the country might become the scene of the "Spanish civil war of the Americas."

The second volume embraces a rich collection of viewpoints from various disciplines. Both books are riddled with errors in repeatedly misspelled names and improperly accented words, although Malloy uses less jargon in his contribution. Thorn gives an optimistic account of economic gains made by the revolution, the first socioeconomic one that the United States has ever supported. James W. Wilkie indicates that Bolivian control of expenditure should have been centralized and extended over the entire public sector of the economy.

Herbert S. Klein summarizes his work on Bo-

livian history to 1952, also presenting the most balanced statement of the fascist aspects of the Villarroel period. The late Carter Goodrich recalls his experiences in La Paz during the 1952 insurrection and the beginning of United Nations technical assistance, replaced later by the American efforts during the Eisenhower and Kennedy administrations.

On agrarian reform William E. Carter estimates that about one-third of Bolivia's agricultural land has been redistributed. Madeline Barbara Léons and William Léons find the impact of land reform in the Yungas area beneficial, although they emphasize that "the position of the *campesino* [Indian farmer] has [not] changed much in relation to other segments of the society." Melvin Burke compares the production of four former haciendas on the Bolivian side of Lake Titicaca with their Peruvian counterparts, finding more favorable results for the Bolivians. He thus reminds us that fragmentation of larger, possibly more economic land units is valid since the former owners did not use much of their vast acreages.

Finally, there is an excellent discussion by Murdo J. MacLeod of long-neglected Bolivian novels of social protest. He wonders if there is "something basically wrong with the Bolivian Revolution" because of the decline in quality of such literature after 1952, and his question goes to the heart of the matter. Has the transformation of Bolivian society been as profound as claimed by most scholars? The resurgence of great Mexican literature since 1910 and that of Cuba since 1959 goes without saying. Augusto Céspedes, one of three Latin American writers invited to the Soviet Union in 1971, decided to stop competing in Bolivia's literary contest in 1961 in order to encourage younger writers. But the genius of Céspedes seems lost. Who among Bolivian youth can sustain his work? They are disenchanted with the reformist trend and corruption of the MNR and its frequently repressive successors. Witness the Teoponte *foco* of 1970 and the resistance at the University of San Andrés a year later. Perhaps beyond the Bolivian Revolution lies the continued agony of Guatemala.

JERRY KNUDSON

South Dakota State University

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## Communications

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*A communication will be considered only if it relates to an article or review published in this journal; publication of such a communication or of any part of it is solely at the editors' discretion. Limitations of space require that a communication concerning a review be no longer than the review to which it refers and in no case longer than 500 words. Communications concerning articles or review articles may be no more than 1,000 words, and the editors reserve the right to impose a lower limit. The schedule of publication and the time needed to send a communication to the author of the article or review in question for such reply as he may care to make virtually preclude the possibility of publication in the issue following that in which the original article or review appeared. Unless, in the editors' judgment, some major scholarly purpose is served, rejoinders will not be published.*

### TO THE EDITOR:

You published a review of the first two volumes of *Histoire générale de la presse française* (AHR, 76 [1971]: 510–11) of which we are the editors. Somewhat surprised at its tenor, we would ask that you permit us to bring to your readers' attention the following remarks. They seem to us an indispensable corrective to the evident bias coloring the verdict returned by Professor Elizabeth L. Eisenstein on a work that has, in general, been given a better reception by other historians than by your reviewer.

Professor Eisenstein is quite entitled not to like this work but one may question the grounds on which she bases her criticism. According to her these two volumes fail "even as a coffee table showpiece," and she "cannot imagine

what group of purchasers they will please." Can your readers accept this summary dismissal without asking themselves how it was that a distinguished group of French academics could have written a work of such little interest or how a famous French publisher could have agreed to publish in several volumes so bad a piece of work? The claim that the reference apparatus of notes, index, and bibliography is very complete but that it aims to "impress the reader without much concern for actual use" will put your readers on their guard. Can they accept the opinion underlying the entire review that this history, appearing in 1969, is so out of date as to contribute nothing more than the old and learned study by Hatin written during the Second Empire? Can they believe that the authors and the publisher are so lacking in good sense that they have "chosen at random" illustrations that "rarely instruct or even entertain"?

In her more detailed comments Professor Eisenstein is still more relentless. Even without discussing all her comments, we must nonetheless reply to some so that your readers may better judge their value. Concerning the episodes in the life of the *Gazette* during the Fronde, the evidence adduced by M.-N. Grand-Mesnil is not as conclusive as Professor Eisenstein wishes to believe, and one cannot reproach M. Louis Trenard for not accepting it entirely. The numerous references the latter makes to Hatin in discussing the press of the *ancien régime* are not proof that he has merely copied it; rather, they should serve as a proof of intellectual exactitude and as an indication, in the many sections where it is not quoted, of the originality of the text.

An attentive reader will find no major con-

tradition between the allusions to the extent of illiteracy at the beginning (p. 159) and end of the eighteenth century (p. 402). It is difficult to understand why Louis Trenard is reproached for having adopted a very clear-cut plan that classifies, for the eighteenth century, titles of journals by category (provincial press, specialized press, and so on) and—above all—by the chronological divisions 1631–1724 and 1724–88 (which, be it noted, apply independently of the “classical” divisions of political chronology) on the pretext that “political history under the *ancien régime* is not helpful in ordering journalistic developments.”

To mention, in conclusion, only one of the criticisms made of the second volume (1814–71), it is surprising to see regrets expressed that the attitude of the Catholic Church toward the press is not discussed when, on the contrary, the book has much to say on this point both when presenting Catholic papers (pp. 70, 104–06, 128, 175, 213–15, 245, 267–69, 277, 325–26, 349) and when discussing religious problems (pp. 82–84, 295–97, 337–38).

To sum up, we consider it regrettable that readers of a periodical as highly regarded as yours must judge a work as important as this—whose next volumes (1871–1940 and 1940–70) will be appearing in the coming months—on the basis of a review that is more polemical than reasoned.

CLAUDE BELLANGER  
JACQUES GODECHOT  
PIERRE GUIRAL  
FERNAND TERROU  
*The French Press Institute,*  
*Paris*

#### PROFESSOR EISENSTEIN REPLIES:

I regret that it was not possible to please the editors and, at the same time, give an accurate report of their work to readers of the *American Historical Review*. I did try to balance the defects of volume 1 against the merits of volume 2 but saw no reason to conceal my disappointment that the project as a whole was modeled along the same lines as its predecessor, a hundred years ago.

There is no need to take up space with a detailed refutation of the specific objections posed by the editors. After comparing my comments

with the editors' complaints, interested readers may judge for themselves whether the latter are well founded or not. Prospective purchasers also would be well advised to ignore appeals to their credulity concerning illustrations and reference apparatus. Those interested in buying these volumes ought to look them over first.

To forestall misunderstanding I am not denying that some scholars may find it useful to own this work. I am suggesting that blurbs, prefaces, and packaging are misleading, and I think purchasers will be less dissatisfied if they have been forewarned.

ELIZABETH L. EISENSTEIN  
*American University*

#### TO THE EDITOR:

Robert Dallek's review of my *Roosevelt: The Soldier of Freedom* (*AHR*, 76 [1971]: 1503–13), strikes me as the very model of a scholarly critique: he has fairly summarized the theme and subthemes of the book, indicated the relevance of a large number of other studies, and clearly stated his own views and assumptions as well as my own. I value both his tributes to my study and his own sharply different evaluation of Roosevelt as a world leader.

Both Professor Dallek and I judge Franklin Roosevelt on the basis of our clearly stated criteria. It may be useful to note, however, that the criteria of presidential leadership in World War II that I advance are not only mine; they are also Roosevelt's. It was Roosevelt who favored and promised—and whose generals planned and pressed for—an early second front; it was Roosevelt who advocated a strong postwar role for China; it was Roosevelt who knew the weaknesses of the League of Nations and wanted a strong United Nations; it was Roosevelt who opposed colonialism; it was Roosevelt who talked about ending the old system of power politics and spheres of interest. While Roosevelt *acted* by and large in the fashion in which Professor Dallek wants presidents to act, he generally *spoke* for the kind of presidential leadership I favor, and it seems fair to evaluate Roosevelt's actions by his own precepts.

Even so, I believe that the historian has a right, if not a duty, to evaluate presidential leadership by clearly defined standards of his

own, which both Professor Dallek and I have tried to do.

JAMES MACGREGOR BURNS  
Williams College

TO THE EDITOR:

I learned much from the two reviews of my *Gandhi's Truth* (Joan V. Bondurant, Margaret W. Fisher, J. D. Sutherland, "Gandhi: A Psychoanalytic View," *AHR*, 76 [1971]: 1104-15). If I respond to one criticism common to both it is only because this may help to correct a widespread misunderstanding obviously caused by lack of clarity on my part.

Throughout my book I refer to the Ahmedabad strike as the Event with a capital E. Although I nowhere say so, this has been interpreted as a claim that I have found *the* key event in Gandhi's life and in the history of *satyagraha*. Actually, the capitalization of the word *event* was meant to be a literary device which would remind the reader that all the theoretical and factual strands pursued throughout the book would eventually converge on the story of the strike. This strike had become *the* event for *my book* and *for me* because industrial Ahmedabad is the Indian city I learned to know the best and some of the old men and women I met there had played significant roles in the strike and, thus, were true witnesses to it. As to the place of this event in Gandhi's life, I do claim that it was more important than has been generally recognized because it was the prophet's first nonviolent campaign in his homeland and, in fact, in his province and language area. Such a claim, after all, causes historical books to be written. But I was not so enamored with my experience and with my role that I would believe I had discovered the key event in Gandhi's long and adventurous life.

ERIK H. ERIKSON  
Harvard University

TO THE EDITOR:

Although I appreciate the difficulties involved in reviewing responsibly the many works that come to the attention of the *American Historical Review*, I must still protest the treatment accorded my *The Social Responsibilities of*

*Business: Company and Community, 1900-1960* (*AHR*, 76 [1971]: 1615-16).

In his pursuit of what must have been remarkably few grammatical lapses the reviewer seems to have substantially misunderstood the title and subject matter of the study, despite the fact that they were quite clearly elaborated in the preface. There it was explained, for example, that the field of labor relations was specifically excluded in order to concentrate on community relations (as the title indicated). One may question the wisdom of that decision, but I cannot accept criticism for overlooking a topic that I deliberately chose to exclude.

On the charge of having shown excessive sympathy for business leadership I am prepared to accept criticism—provided it is substantive and not merely assertive. I suspect myself that this charge may not be without foundation, although it should be noted that the book has been criticized elsewhere for being too unfriendly to business. My endeavor was to approach the material from the point of view of the executives themselves (as explained in the preface) in order to understand the factors that influenced their thought and behavior. This demands a modicum, at least, of sympathy; and it can hardly be argued that my assessment was uncritical.

I should have welcomed, finally, your reviewer's suggestions as to how I might have treated Ralph Nader in a volume whose time span (indicated in the title) ends about 1960. There is a difference between historical scholarship and contemporary polemics (referred to in the preface) that seems to have escaped him.

MORRELL HEALD  
Case Western Reserve University

PROFESSOR DIBACCO REPLIES:

I had no difficulty in reviewing fairly Professor Heald's book. My review was designed to convey both the merits and shortcomings of his research and writing. That the latter outweighed the former, in my opinion, is naturally disturbing to Professor Heald, but my evaluation does not constitute irresponsibility any more than reciting the Rotary motto each morning ("He profits most who serves the best") constitutes business responsibility.

Grammatical and stylistic deficiencies, unlike



wild game, need not be pursued; they have a way of making themselves known to readers. Professor Heald establishes for himself the same wide margin for error that he sets for his business spokesmen—and I am critical of such low standards.

The problem with the title is no big thing. I simply feel that a book title should not confuse the reader. *The Social Responsibilities of Business* infers that business units and their noneconomic relations with the outside world are under consideration. Professor Heald's addition of a subtitle (exclusive of dates) does not, in my opinion, convey the precision of meaning that was intended.

In my allotted four hundred-word review I did give what I believed to be substantive criticisms. Alas, Professor Heald has interpreted these as reflective of my unhistorical, contemporary viewpoint. I did not expect Professor Heald to include Ralph Nader in his study, but I do expect that a historian writing in 1970 would show more discrimination in examining the business record than Calvin Coolidge or Émile Coué.

THOMAS V. DIBACCO  
*American University*

#### TO THE EDITOR:

I very much agree with the tenor of Felix E. Hirsch's review of the memoirs of Heinrich Brüning (*AHR*, 76 [1971]: 1560–62), but I differ from his opinion in one important point, and I think a few aspects need stressing that Professor Hirsch has not mentioned.

Hirsch's review, though generally very fair to Brüning, implies an accusation when he mentions Brüning's intention to replace "his own moderate government . . . by a rightist coalition headed by Carl Goerdeler." This was not a matter of free choice for Brüning; he was aware that President Hindenburg would not indefinitely support the Brüning cabinet, which was based on toleration by the Social Democrats and which conducted a policy making such toleration possible. Under these circumstances Brüning wanted to make sure that the chancellorship would not be handed over to any rightist who might stage a coup d'état. In spite of Goerdeler's very great limitations, it would be hard to fault Brüning's judgment that among

those whom Hindenburg might conceivably be persuaded to appoint, Goerdeler was the least objectionable.

Hirsch mentions Brüning's intention to reintroduce the monarchy, but he does not draw the reader's attention to the new light thrown by the memoirs on this plan, which itself has been known for a long time. Most historians, I think, have up to now assumed that Brüning, although always a monarchist in the abstract, formed a concrete plan of restoring a Hohenzollern to the throne only as a desperate means to keep Hitler out of power (the same reason a number of Bavarian politicians in 1933 considered the restoration of the Wittelsbach monarchy), believing that the Social Democrats and other nonmonarchists would accept the restoration because, from their point of view, a Hohenzollern king was still preferable to the Nazis. Although the latter point was incontestable, even such a plan would have been unrealizable. But according to his memoirs Brüning did not think of the restoration as a crisis solution; rather, he wanted the monarch to come in after the crisis, when he himself would have found a solution for the foreign policy problems and would thereby have achieved economic recovery, abatement of civil commotion, and especially a waning of the power of the Nazis. The restoration should by no means be effected through counterrevolution but in a constitutional manner with the acclaim of the public and especially with at least the tacit consent of the working class. If a restoration of the monarchy as a crisis solution was unrealistic, this idea of a post-crisis restoration was sheer fantasy. Why should the great majority of Germans, who were not monarchists, voluntarily accept a change of regime after the most pressing problems had been solved—a change desired merely by conservative bureaucrats, big landowners, some industrialists, army officers, and a few political romanticists like Brüning himself?

The restoration plan reveals a deep contradiction in Brüning's mind. Not only his foreign policy (at least in parts) but also important aspects of his handling of domestic affairs—the way he established tolerably good relations with the moderate Left, which he had first driven into opposition in 1930—show Brüning as a leader of statesmanlike qualities; for example, his assessment of Pius XII and his rejection of

the idea of a federal concordat show a remarkable degree of realism. On the other hand, there was this absurd restoration plan and the overestimation of the consolidating effect that would come not only from a reparations agreement but also from German equality in armament, which could satisfy only nationalistic emotions but had no bearing on Germany's practical problems. In the same category belongs Brüning's belief that his government by emergency decrees, which was acceptable to public opinion only as long as desperate conditions prevailed, amounted to a constitutional reform that would permanently give the president the position of a monarch, limited only by the purse-string powers of parliament. All this offers evidence that in addition to Brüning's world of realistic political labors, in which he acted with an admirable combination of shrewdness and honesty, he also cultivated a romantic dream world in which he had no ground under his feet.

The heavy mortgage romantic emotionalism placed on Brüning's statesmanship may provide an answer to another question. The memoirs contain nothing about Brüning's activities in the United States. He was the only one among German refugees from Hitlerism who had any chance at all to be listened to by people in the American government. Did he never try to exert some influence in opposition to the Morgenthau tendencies? The lack of evidence for any such attempt seems to indicate that there was none; correspondences I had with Brüning in 1941 and 1943 support the same assumption. Still there is no proof one way or the other, but what we know makes it likely that Brüning did not try to influence American foreign policy. Most probably even Brüning would have achieved nothing; yet he might have tried to reach the seats of power through Catholic circles with some influence in Washington. Surely a number of German refugees (Albert Grzesinski, Gerhart Seger, and others) would have made such an attempt if that channel had been open to them. Assuming that Brüning really kept aloof, my guess is that he realized the hopelessness of any plea for a non-Carthaginian peace for Germany, unless such a plea was combined with proposals like German disarmament and reparations for the nations victimized by Hitler, and that it was emotionally impossible for Brüning to advocate such conditions, although un-

der challenge he would surely not have denied their justification.

CARL LANDAUER  
University of California,  
Berkeley

#### PROFESSOR HIRSCH REPLIES:

I am glad that Carl Landauer and I agree fundamentally in our appraisals of Heinrich Brüning as we know him now from his *Memoiren*. Some differences of opinion that Landauer presents deserve a brief reply, however.

Professor Landauer is correct in saying that, if there was to be a rightist coalition government in June 1932, Carl Goerdeler would have been perhaps the least objectionable politician to head it. But this is a very limited compliment. As his biographer Gerhard Ritter told, Goerdeler wanted to negotiate at that point with Hitler and to offer him two or three seats in the incoming cabinet. If the Reichstag were to cause trouble, Goerdeler suggested dissolving it and having new elections two years later only. While Goerdeler was certainly a much more honorable man than Herr von Papen, I maintain he, too, would have been a very dubious chancellor for a democratic republic at such a critical juncture. Brüning, by the way, later realized Goerdeler's great shortcomings as a statesman.

I would have said more about Brüning's disastrous ideas concerning a restoration of the monarchy had the space allotted to my review permitted it. We knew from Sir John Wheeler-Bennett's *Wooden Titan* (1936), the still unsurpassed Hindenburg biography, that Brüning had seriously thought of this restoration when his regime began to falter in November 1931. But Brüning admits in his *Memoiren* (p. 146) that he had not raised fundamental objections to a restoration of the Hohenzollern rule in a discussion with General von Schleicher as early as Easter 1929, that is, long before he took office, when the Weimar Republic had not yet entered the crisis stage and when Hermann Müller's cabinet of the Grand Coalition seemed fairly stable. Later, as chancellor, it was usually Brüning, rather than Hindenburg or Schleicher, who brought the topic of restoration up for an exchange of views. Hindenburg's disloyalty to the republic is a matter of record, but Brüning, un-

til now, seemed to have been its faithful servant. Had those of us who publicly supported Brüning's campaign for Hindenburg's re-election in 1932, in spite of serious doubts about the old field marshal, known that neither of them intended to protect the Weimar Constitution, we might have hesitated to follow the chancellor's leadership. He certainly withheld his true intentions then from the German people. Most reluctantly, we may have to accept the opinion of the German historian Karl Otmar Freiherr von Aretin (*Frankfurter Hefte*, 26 [1971]: 931-39) that the real historical significance of Brüning rests in the fact that by his *Verfassungs-Untreue* he made the most important constitutional institutions of the republic ready for their destruction by Hitler.

Finally, Professor Landauer raises the question of Brüning's political role while he was in this country. He seemed rather aloof to many of his Harvard colleagues, even to a man as sympathetic to German culture as Sidney Fay. From understandable motives, the former chancellor did not choose to become an American citizen, a fact that hampered his activities in wartime. Nevertheless, he did remain deeply concerned about Germany's future. George N. Shuster, former president of Hunter College, has eloquently described Brüning's American years in his contribution to the Brüning *Festschrift* entitled *Staat, Wirtschaft und Politik in der Weimarer Republik* (Berlin, 1967). I can perhaps throw some additional light on the subject. What few people knew was that he had contacts with certain political exiles from Nazi Germany. While he disliked many refugee intellectuals, he trusted a few former Social Democratic statesmen and writers. Among them was William Sollmann, the former SPD leader in the Rhineland and minister of the interior in the Stresemann cabinet. Sollmann, a close friend of mine (see my essays on him in the *South Atlantic Quarterly*, 52 [1953]: 207-27, and in the *American-German Review*, 19 [1953]: 14-16, now supplemented by Eugene Kist's article in *Quaker History*, 60 [1971]: 88-119) had told me repeatedly of his extensive contacts with Brüning. We have a record of them in Sollmann's papers, now deposited in the Swarthmore College Peace Collection. They confirm that Brüning made serious, if unsuccessful efforts to save Socialist leaders like the former

minister Rudolf Hilferding from falling into the hands of the Nazis. Brüning refers repeatedly to wartime visits paid to him by a prominent emissary of the State Department and the OSS. He made it amply clear in his letters that he did not believe that refugees like himself could return to office after the war. In 1944, however, he suggested for an Allied occupation of Germany the formation of a small advisory committee of emigrants who had become citizens of Allied or neutral countries. Among them he named Sollmann, Friedrich Stampfer, Max Brauer, and Gerhart Seger, four moderate Social Democrats. Also a few Democrats should be included, but he reiterated that he could not belong to it himself, since he was not a U.S. citizen. He described his own frustrations during the American years to Sollmann in a letter of December 16, 1946: "In spite of my avoidance of Washington after my experience in 1939 with President Roosevelt and the intrigue in the summer of 1944 to prevent very influential people from discussing the Morgenthau proposals with me before the Quebec conference, people are alarmed even now by the possibility of my returning into politics." A few years later Brüning went back to Germany, but most of his countrymen cared but little for his advice, and he ended his days, a discouraged old man, in a little village in Vermont.

FELIX E. HIRSCH  
Trenton State College

#### TO THE EDITOR:

I have only just seen the review by Robin Higham of *Documents Relating to the Naval Air Service*, volume 1 (*AHR*, 76 [1971]: 508-09), which I edited for the Navy Records Society's series. I welcome constructive criticism of my published work from any well-informed person. But I take strong exception to Mr. Higham's remarks, which are in some cases quite inaccurate and in others totally unjustified. An author or an editor of original papers is entitled to expect reasonable care and knowledge on the part of a reviewer and that he should have read the work he is discussing. Mr. Higham obviously has not fulfilled those expectations.

As to the date of the formation of the Royal Naval Air Service Mr. Higham has evidently repeated a mistake made by several writers. A

Royal Warrant is not an executive order, as he seems to suppose. It is merely an authorization. To quote the definition in the *Oxford English Dictionary*, a warrant is "a document conveying authority or security. A writing issued by the Sovereign, an officer of state or an administrative body authorizing those to whom it is addressed to perform some Act." The Admiralty, which was the body concerned with the Royal Warrant of July 1, 1914, did not act on it until thirteen months later. Indeed on July 1, 1914, the Admiralty issued a statement referring to the RNAS as "forming the Naval Wing of the Royal Flying Corps" (see doc. 47 in my book). Obviously then it was not a separate service on the date in question. On July 29, 1915, an Admiralty Weekly Order was issued stating that "the Royal Naval Air Service is to be regarded in all respects as part of the Royal Navy. . . . *These instructions will come into effect on 1st August 1915*" (italics supplied; see doc. 72 in my book). Hence the effective date of the execution of the Royal Warrant and the establishment of the RNAS is clearly established.

With regard to Rear-Admiral Murray Sueter, as I described him as "brilliantly inventive" (p. xi), which is almost verbatim what Mr. Higham says, and I also quoted Sir Walter Raleigh's tribute to him (p. 57), it is nonsense to suggest that I treated him unfairly. With regard to what Mr. Higham calls his "demise" (presumably meaning his replacement at the Admiralty in 1917), scholars will surely prefer the documents I published (docs. 150, 151) to what Sueter told Mr. Higham in 1959 when Sueter was eighty-eight years of age and within a year of his death. Nor are my remarks on Sueter's conduct undocumented, as Mr. Higham states. Documents 48 and 81, among others, are highly relevant.

For Mr. Higham to pick on my footnote about Pemberton Billing, a quite insignificant character of the period, as inadequate when I gave Billing six lines of small print is petty to the point of absurdity. Mr. Higham may like more and longer footnotes, but the NRS depends entirely on voluntary subscriptions and on its editors' giving their services free. It simply cannot afford the printing costs of long and copious footnotes. That is why I put nearly all my plentiful editorial comments and explanations in the text and indicated them by heavy

square brackets—an aspect of this book that Mr. Higham ungenerously but totally ignores.

As to books on the period, I cannot see the relevance of Mr. Higham's remark, unless it is inspired by pique at my failure to mention his work on British airships. If he had troubled to read the inscription on the flyleaf he would see that the NRS is described as existing "for the purpose of printing rare or unpublished works for naval interest"—not to quote from published books.

I will pass over Mr. Higham's concluding remark about my knowledge of the period by merely describing it as a patronizing and gratuitous insult, which should not be made by a reviewer even of a trivial work. Though I would be the last to claim complete knowledge of this or of any other period I very much doubt whether Mr. Higham can teach me much about my subject—as he evidently believes he can.

I hope that the *AHR* will find a better-informed and more fair-minded reviewer for the second volume of the work in question.

S. W. ROSKILL

*Churchill College, Cambridge*

#### PROFESSOR HIGHAM REPLIES:

Let me respond to Captain Roskill's criticisms of my review by first denying his assumptions that I am not qualified to review the work and that I did not read it.

Surely when an editor discovers important evidence, in this case that the establishment of the Royal Naval Air Service was not on the date commonly accepted by the official historians of the First World War, by naval historians, and by myself in *The British Rigid Airship* (1961), he has some duty to point this out in his introduction rather than making a bald statement and then providing the explanation in a letter to the editor of a journal in which he is faulted by a reviewer.

On the question of Rear-Admiral Sir Murray Sueter's demise at the Admiralty, I would simply respond that in cases of this sort the written record seldom tells the whole story. While it is quite true that at the time I interviewed Sir Murray he was advanced in age, his memory was quite accurate, a fact I could judge by having worked through the documentation then available, which unfortunately excluded

much of the Admiralty materials now revealed, before I saw him.

Captain Roskill should well recognize that reviewers have very limited wordage assigned. I drew attention to the footnote on Pemberton Billing simply as an example of the problem, feeling it better to be specific rather than make a sweeping charge. It is unfortunate that the Navy Records Society cannot afford better notes. One can sympathize with them on this. My basic point, however, remains that as the period recedes into the past the reader needs better identification not only of people, but also of expressions, items, and equipment. As to Captain Roskill's brackets in the text, I have rechecked, as a sample, pages 320 to 420 and find these to be simply routine linkages or explanations of the sort editors normally supply. In

this respect the notes seemed not to merit special mention in a short review, though undoubtedly they help provide continuity.

Finally, Captain Roskill obviously does not understand that bibliographies should, if they are to be of any use, guide the interested person to the relevant material on the subject. In this sense they reflect within limitations an author's or editor's knowledge of the subject. I certainly did not expect him to quote from my book, which is a monograph (devoting most of the first 229 pages to the naval airship program) and not a collection of documents. For my part, I am perfectly aware of the work of the NRS and have supported it with a standing order from our library.

ROBIN HIGHAM  
*Kansas State University*



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## Recent Deaths

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On September 19, 1971, WILLIAM FOXWELL ALBRIGHT, one of the great scholars of this century, died in Baltimore, Maryland. A giant in the field of Near Eastern and Biblical studies, his productivity was immense: in the six decades following the publication of his first article in 1911, the total number of his books, articles, and reviews exceeded one thousand. Universally acclaimed for his contributions to knowledge, he was the recipient of countless honors from academic institutions and societies, both foreign and domestic.

W. F. Albright, whose parents were Christian missionaries, was born in Coquimbo, Chile, on May 24, 1891. Educated in the United States, he received his B.A. from Upper Iowa University at Fayette in 1912 and his Ph.D. from Johns Hopkins University in 1916. The year 1919 found him in Jerusalem, a fellow of the American School of Oriental Research; by 1920 he was acting director of the school and subsequently its director (1921–29). Professor of Semitic languages at Johns Hopkins from 1929 until his retirement in 1958, his association with the American School in Jerusalem continued, and he served a second term as director (1933–36). Albright remained active in teaching, research, and lecturing after retirement until struck down by the massive stroke in July 1971 that carried him off two months later.

An archeologist, a historian, a paleographer, an epigrapher, and a profound student of ancient languages, Professor Albright was no narrow specialist; it should also be emphasized that he was the master rather than the jack of all these trades. His publication of the results of the excavations at Tell Beit Mirsim (3 vols., 1932–43) established the fundamental chronol-

ogy for Palestinian archeology. A more popular treatment of the subject, which became for many students their first introduction to the field, was his *Archaeology of Palestine*, which appeared as a Penguin paperback in 1949. Among his other well-known works one might cite *Archaeology and the Religion of Israel* (1942) or *Yahweh and the Gods of Canaan* (1968), but the true measure of the man as historian and synthesist must be his magnificent, stimulating *From the Stone Age to Christianity* (1940).

A great teacher, Professor Albright had scores of students, many of whom attained scholarly distinction of their own; certainly not the least of these was the late Nelson Glueck who began his study of archeology with Albright in Palestine in 1928. Not only those formally enrolled as his students but also many young scholars who came in contact with Professor Albright were the beneficiaries of his generous advice and encouragement. His kindness and consideration will never be forgotten.

It is good to know that the attainments of this great man were fully recognized before his death: a volume of essays in his honor signaled his retirement and another volume his eightieth birthday; the American School in Jerusalem was renamed the Albright Institute of Archeological Research. Since his death there have been numerous memorials: the most moving of these was conducted at St. George's Cathedral in Jerusalem on September 23, and the testimonials are printed in the *American Schools of Oriental Research Newsletter* (1971–72).

William Foxwell Albright was more than an Orientalist or an ornament to Biblical studies. He was a man to admire; genuine, honest, and

fine. He wished to be known as a Christian humanist, and he was truly that.

TOM B. JONES  
*University of Minnesota,  
Minneapolis*

PHILIP E. MOSELY, Adlai E. Stevenson Professor of International Relations, director of the European Institute, and associate dean of the School of International Affairs at Columbia University, died after a long illness at his home in New York City on January 13, 1972.

Professor Mosely was born September 21, 1905, in Westfield, Massachusetts. He received his B.A. from Harvard University in 1926 and his Ph.D. in 1933, benefiting from the guidance of men such as Archibald Cary Coolidge and William L. Langer in diplomatic history and Michael Karpovich in Russian history. He spent from 1930 to 1932 in the Soviet Union, beginning then the acquisition of his unsurpassed knowledge of the Soviet land and peoples. The opportunity to spend 1935-36 in the Balkans and other summer travels throughout Eastern Europe helped give him the extraordinary fluency in Romanian, Russian, and Bulgarian, which, together with equal mastery of the West European languages, helped to increase his understanding and his ability to communicate.

Professor Mosely's formal academic appointments were all with Eastern institutions: Princeton, Union College, Cornell, Columbia, and the Council on Foreign Relations. From 1942 to 1946 he occupied important positions in the Department of State, serving as adviser to Secretaries of State Cordell Hull and James Byrnes. His participation in the World War II settlements, notably those at Moscow and Potsdam, increased his insight and interest in Soviet foreign policy and Soviet-American relations. It also drew him from his earlier interest in nineteenth-century diplomatic history and Balkan social history into a prominent role in helping to shape American foreign policy, because he remained an important adviser to the Department of State after 1946. Thus the exciting "open skies" proposal of President Eisenhower was his suggestion. The relationships Professor Mosely maintained with Soviet and East European scholars and diplomats were sustained throughout the crises of the past quarter century. This unusual feat reflected the respect in

which they held his knowledge, understanding, judgment, candor, and integrity. He would have served with distinction as our ambassador in Moscow or in any of the East European capitals if he had not committed himself to academic work after 1946.

As a scholar Professor Mosely was remarkably prolific. His works reflect high quality in spite of their variety and his numerous activities. His first book, *Russian Diplomacy and the Opening of the Eastern Question in 1838 and 1839* (1934), was a model of diplomatic history. His translation and editing of Victor Chernov's *The Great Russian Revolution* (1936) made available a significant interpretation of the Russian Revolution that might otherwise have been ignored. Professor Mosely excelled, however, as an author of articles, publishing more than two hundred of originality and insight on subjects that reflected his wide interests and vast learning, articles ranging from Bulgarian village life in the nineteenth century to the history of Slavic studies. In the last year of his life he planned to return to study of the *zadruga* or communal joint-family in the Balkans, an interest of his since the early 1930s, and to transform a half dozen articles into the first full study of this central but little-known social institution of Southeast Europe.

Professor Mosely was not an outstanding lecturer but he will be remembered as a great teacher. His graduate seminars were entirely dedicated to passing on the skills of the craft. His work on dissertations, those which he directed and those which were the basic responsibility of colleagues, was prompt and extraordinarily careful. Indeed, he devoted the spring of 1971, his first sabbatical and one taken to help him recover from several painful operations, to his usual careful analysis of theses. Mosely also made counseling his own students and others an art. He always found time and energy to give advice and help, when asked, to students and colleagues who needed technical assistance, advice concerning fellowships or positions, or simple solace. His concern for all students, his almost intuitive understanding of them, and his devotion to the academic community all help explain the fruitful role he was able to play when Columbia was torn by dissension and troubles in 1968.

Professor Mosely was also one of the first to

recognize that teaching is not limited to the classroom. He was very active with businessmen's groups interested in international politics. He felt an especial obligation to meet foreign scholars of all political persuasions in international conferences, and he was eager to consult with scholars from other countries, especially in Eastern and Western Europe and in Asia, concerning means of improving higher education and international understanding.

Professor Mosely's greatest contribution, however, was in expanding and improving research and instruction concerning Russia and Eastern Europe and in promoting multidisciplinary study. He was one of the original members of the famed Joint Committee on Slavic Studies, established by the Social Science Research Council and the American Council of Learned Societies in 1937, which has served as the principal planning and coordinating body for American study of Russia and Eastern Europe. This group not only brought major scholars together in a cooperative program, but it created a spirit of joint endeavor that helped the Russian field to spread easily into new centers of study and to avoid the shattering conflicts that divided Chinese studies so bitterly in the 1950s.

Professor Mosely was one of the founders of Columbia's Russian Institute, which since 1946 has been the model for training scholar-teachers in all non-Western areas. He served as director of the institute from 1951 to 1955, when he left Columbia for eight years to become director of studies at the Council on Foreign Relations, where his work at a more advanced level helped to stimulate research and the publication of a series of volumes on China and Western Europe.

His services extended far beyond the usual confines of even a great university. Impressed by our need for more information and by the plight of refugees from the Soviet Union and Eastern Europe, he helped to organize and administer the Research Program on the USSR and the East European Fund, which from 1951 through 1961 and 1952 through 1961, respectively, enabled hundreds of refugee scholars to continue their studies and to make the transfer to American academic life. He helped establish the Chekhov Publishing Company, which published the works of refugee scholars as well as Russian classics then unavailable in Soviet editions. Professor Mosely somehow found time to

establish a Russian archive at Columbia University, which has already become an important repository for manuscripts and memoirs, both written and recorded on tape. He served as a consultant for both the Ford and the Rockefeller Foundations and on the boards of trustees of organizations such as the Foreign Policy Corporation and the RAND Corporation.

In short, Professor Mosely enjoyed a full, successful, and satisfying life of service. He was acknowledged for his labors by honorary degrees from the University of Notre Dame, Union College, and Middlebury College. Above all, he won the respect and affection of hundreds and even thousands of American and foreign scholars for the immense learning and understanding he carried so lightly, for his keen, analytical intelligence, for his warm interest and compassion, and for his absolute integrity and decency.

ROBERT F. BYRNES  
*Indiana University*

WILLIAM L. NEUMANN, professor of history and chairman of the American Studies program at Goucher College, died on September 30, 1971, at the age of fifty-six.

Born in Buffalo, New York, he earned a bachelor's degree from the State Teachers College in Buffalo and then a master's degree and a doctorate from the University of Michigan. A pacifist and conscientious objector during World War II, he served in lieu of military service as a forest ranger in California and Massachusetts, as a lumberman in northern Michigan and Oregon, and then for a year as a human guinea pig at the University of Pennsylvania Medical School. At the end of the war he joined the faculty of the University of Hawaii, leaving there in 1949. That year he became executive secretary of the Foundation for Foreign Affairs and editor of *American Perspective*, a quarterly journal of foreign affairs. He first came to Goucher in 1952 while serving as staff consultant on foreign affairs for the U.S. Senate Republican Policy Committee. By 1958 he was a full professor at Goucher. He also taught at Howard University, the University of Virginia, the University of Maryland, the University of Wisconsin, Temple University, and Morgan State College.

His distaste for war carried over into his scholarly work. A revisionist historian in the

tradition of Charles Beard, he was a constant critic of U.S. foreign policy. His role as critic took him into several fields of endeavor: speaking before various organizations, radio and television appearances, and membership in organizations such as the Society for a Sane Nuclear Policy, the Conference on Peace Research, the Society for Historians of American Foreign Policy, the American Civil Liberties Union, the American Historical Association, and the American Association of University Professors.

Author of many scholarly articles he also wrote two books: *America Encounters Japan: From Perry to MacArthur* (1963) and *After Victory: Churchill, Roosevelt, Stalin and the Making of the Peace* (1967). A popular and dynamic teacher, his sudden death saddened a host of friends.

GEORGE A. FOOTE  
Goucher College

ALLAN NEVINS, former president of the American Historical Association, died on March 5, 1971, after a varied and distinguished career of some sixty years in journalism, teaching, writing, scholarship, and academic and public service. Born May 20, 1890, on a farm outside Camp Point, Illinois, of Scottish and German forebears, Allan Nevins learned early the cardinal virtues of duty, industry, thrift, perseverance, and probity—qualities he displayed in exemplary form the rest of his life. In 1908 he entered the University of Illinois where he studied English literature and edited the college paper. He stayed on for a year after graduation to study under the brilliant Stuart Sherman and to write the first of his many books, a biography of Robert Rogers. Professor Sherman recommended the young literary critic to friends in New York City; in 1914 Nevins moved to that city, which remained thereafter his spiritual and intellectual home, and took a job as editorial writer on the *Evening Post*—whose history he promptly wrote. He was now fairly launched on that editorial career which was to command his lifelong allegiance, serving successively as editor on the *Post*, literary editor on the *New York Sun*, and editorial writer under Walter Lippmann on the *Morning World*. Nevins's abiding interest in journalism was attested by his history of the *Post*, a collection of *American Press Opinion from Washington*

to Coolidge (1928), several volumes of collected editorials by Walter Lippmann, and contributions to the *Dictionary of American Biography* on newspapermen and journalists, which together constitute a compendious volume. Somehow amid the exacting demands of editorial work Nevins found time to produce, in 1924, a history of the *American States during and after the American Revolution*, which was awarded the first of many prizes he was to receive in his long literary life and which remained, for almost half a century, a standard and almost a classic work in its field. Next year came the first of several versions of a biography of John C. Frémont, and in 1927 he wrote a volume in the new History of Social Life series, *The Emergence of Modern America*, which foreshadowed a lifelong interest in the Reconstruction era of American history.

Now fully—though not exclusively—committed to a life of scholarship, editor Nevins became, in 1927, Professor Nevins of Cornell University. Within one year, however, he had returned to New York City to join the faculty of Columbia University, an institution to which he was married and faithful for the rest of his long life.

Professor Nevins was now fairly launched on what proved to be the most productive scholarly and teaching career of any American historian, for none other of this century produced so many major books and so many major students and disciples as this unassuming scholar who was himself innocent of any academic study of history and who so conspicuously lacked the imprimatur of the Ph.D. Now that he could devote full time to scholarship, Nevins turned his cascading energies to the rewriting of much of American history; what was perhaps most remarkable is that he managed to write equally well for a scholarly and a popular audience; his scholarly works were written in vigorous graceful prose and his popular articles, essays, and reviews with a scrupulous regard to scholarly standards. He did not confine himself to any one specialty or area of history but was equally at home in—and productive in—biography and political, diplomatic, military, economic, social, and cultural history. His study of Frémont was followed by major biographies of Henry White (1930), Grover Cleveland (1932), Abram Hewitt (1933), Hamilton Fish (1936),

and Herbert Lehman (1963). Three biographical studies proclaimed Mr. Nevins's abiding interest in the history of business—a two-volume biography of John D. Rockefeller (1940), which was in effect a history of the Standard Oil Company and the Rockefeller Foundation; an essay (written with Jeanette Mirsky) on *The World of Eli Whitney* (1952); and three volumes (written with Frank E. Hill) on *Henry Ford: the Time, the Man, the Company* (1954–63), a full-scale history of the Ford Motor Company and even of the automobile industry in America. Two volumes in the new Yale Chronicles of America series—in addition to innumerable articles—attested a lifelong concern with foreign policy. *The Gateway to History* (1938), designed to invite amateurs as well as to instruct professionals, remains perhaps the most luminous introduction to the study of historiography; and a small study, *State Universities and Democracy* (1962), managed to say something original about American higher education.

In 1945 Professor Nevins decided to concentrate his major energies—for he could never resist the temptation of forays into other interesting fields—on a rewriting of James Ford Rhodes's history of the United States in the Civil War era. To this work, which he planned to span a period of roughly thirty years and which he expected would require twelve volumes, he gave the felicitous name *The Ordeal of the Union* (1947–71). He lived to complete eight of the volumes, carrying the story from the close of the Mexican War to Appomattox. *The Ordeal of the Union* provided a bridge from the old to the new history. Narrative in form, based on exhaustive research in newspapers, manuscript, and archival materials, rich in original interpretations, and presented in a style always vigorous and lucid and often eloquent, *The Ordeal of the Union* was closer to the great narrative histories of the nineteenth century—those by Parkman, Henry Adams, and Rhodes—or to those English models Mr. Nevins so admired—those by Macaulay, Lecky, Churchill, and the two Trevelyan—than it was to the new technical history that (with his encouragement) many of his own students were already writing. Allan Nevins was in many ways a stout traditionalist, but he was also—witness his sponsorship of oral history—an innovator, and one of his last articles celebrated the new techniques which were even then transforming much of

the conventional history that he had written.

Not content with this prodigious output of original work, Professor Nevins undertook editorial activities sufficient to occupy the full time of most scholars. While still editor on the *World* he wrote a minor classic of social history—*American Social History as Recorded by British Travelers* (1923). In 1927 he began to make available some of the more famous diaries of American history: that of the New York City social leader, Philip Hone, in two volumes; those of Presidents Polk and John Quincy Adams in single volumes; two volumes of the journals of Toledo's Brand Whitlock; and, most valuable of all, the massive four-volume diary of George Templeton Strong. Alongside these stand a dozen or so other collections of letters and public papers—the previously mentioned volumes of Walter Lippmann's editorials, the letters of Grover Cleveland and Abram Hewitt, and a selection from the public papers of John F. Kennedy. And during the whole of Nevins's academic life he exercised close editorial supervision over several major series: the American Political Leaders series, the new Chronicles of America series, the Nations of the Modern World series, Heath's College and University History series, and, in the last decade of his life, the fifteen-volume Civil War Centennial series.

This most industrious of scholars was also the most dedicated of teachers. For thirty years he lectured to large classes of graduate students and, in his overcrowded seminars, guided literally scores of others through dissertations and into scholarly careers—dissertations which he supervised and edited with meticulous care, careers which he encouraged with ceaseless benevolence, for his relationship with his graduate students was always *in loco parentis*. His teaching was not confined to Columbia University. He lectured widely, taught for one year in the universities of Australia, and twice held the Harmsworth Chair of American History at Oxford University, where he managed to penetrate the formidable barriers of traditionalism and introduce some long-needed reforms.

Professor Nevins had a third career, which might be characterized—in no pejorative sense—as entrepreneurial. Ceaselessly concerned for the reputation and well-being of Clio, he turned much of his energy to celebrating her virtues, protecting her from those he thought her enemies, and advancing her cause. There was



a Napoleonic quality about Nevins's combination of grand strategy and tactics in these undertakings. We can recall him looking out from his eyrie on the sixth floor of Fayerweather Hall—that vast book-lined and paper-strewn room where the very air vibrated with his energetic presence—looking out not merely over the campus of Columbia University but over the whole broad realm of history and planning forays and excursions to assure its prosperity and its triumph. Thus he launched a campaign to build up the history collections of the University library and brought it to a fortunate conclusion by obtaining for the University the munificent Frederic Bancroft Fund. Thus he campaigned to introduce more American history into the high schools, and he succeeded in influencing legislation everywhere in the nation. Thus he championed the potentialities and the dignity of business history; in his own voluminous writing and those of his associates and students and in the creation of business archives, he did much to give respectability to that heretofore neglected enterprise. Thus he worked long to establish a popular journal that would present American history as *Harper's*, the *Century*, and *Scribner's* of the late Victorian era had done; though he had little help here from his professional colleagues, he succeeded in founding the Society of American Historians and launching the now widely read *American Heritage*, whose counselor he remained to the end of his life.

Doubtless most important of all was Nevins's role in creating—or reviving—modernizing, systematizing, and institutionalizing oral history. Oral history itself was as old as Homer and the Icelandic sagas; what Professor Nevins envisioned was a systematic and professional exploitation of personal recollections of men and women who had played public roles. What would we not give, he used to ask, for Washington's account of his services in the Revolution, for Lincoln's detailed observations on his presidency. It was an argument that persuaded President Truman, and scores of others, to provide thousands of pages of oral history to eager tape recorders. The Bancroft Fund enabled Mr. Nevins to inaugurate this project at Columbia University; soon he had worked out an effective technique and trained a corps of younger scholars in that technique; soon the oral history project developed into a formidable and in-

valuable archive. The idea spread from academy to academy, to private associations like the medical associations and the Red Cross, to government bureaus, and to corporation offices until within two decades oral history became a new dimension of historical research at home and abroad.

In 1958 Mr. Nevins retired, formally, from Columbia University and took the post of senior research associate at the Huntington Library. There he devoted his still abounding energies to building up the library and manuscript collections of that institution, helping to make it pre-eminent in many fields of history and literature, and to his own writing. He was, by long training, a bookman and a book collector; his private library, much of which he disposed of in his own lifetime, numbered well over twenty-five thousand volumes.

Allan Nevins had a distinguished public as well as scholarly and academic career. During the Great War he served as cultural attaché to the United States Embassy in London, and later was a kind of cultural ambassador to Australia. He was an adviser to presidents and statesmen—General Eisenhower, Cordell Hull, Herbert Lehman, John G. Winant, Adlai Stevenson, and John F. Kennedy among them. He rescued the Civil War Centennial Commission from the hands of antiquarians and party hacks. He served, at various times, as president of the Society of American Historians, president of the American Academy of Arts and Letters, fellow of the New-York Historical Society, and historical adviser to the Sixth Fleet. Nor should we fail to recall that shortly after his retirement from Columbia University he gave that institution, out of his literary earnings, half a million dollars to endow a chair of American economic history—a chair that now bears his name.

In all this Allan Nevins sounds like an institution and, indeed, had he not been so ebullient and so dynamic, observers might have mistaken him for that. No mere formal account of his career does justice to his affluent personality—to that enthusiasm for learning so contagious that few could resist its importunities; to that single-minded practicality which had no time for academic dalliance but concentrated on getting results; to that tireless tenacity which overcame obstacles of nature and of human nature and brought his own enterprises and those of his students to fulfillment;

to that generosity, moral and intellectual even more than material, which left no room for envy or malice (in almost forty years of intimacy I never heard Allan make a malicious remark about a fellow scholar); to that homespun simplicity and unpretentiousness which permitted him to take on whatever tasks came to hand that he thought worth doing, to accept old and young with equal fellowship, to extend help to amateurs as readily as to fellow scholars; to a capacity for friendship and affection which brought him the devotion of friends in almost every segment of society—academic, journalistic, political, business, military, and merely neighborly. Somehow Nevins found time, too, for an immense correspondence, not only with fellow scholars and graduate students, but with the exalted world of statesmen, with the fluctuating world of business, with editors, novelists, poets, social workers, and old friends. A collection of some fifty thousand of these letters now awaits the student of American cultural history in the archives of his beloved university.

The pattern of Allan Nevins's private, social, professional, and public life was ever harmonious. That same harmony will be found in the historical monument he left behind him—a monument that will, for many years, cast its long shadow across the historical landscape which he surveyed, explored, cultivated, and embellished with such devotion and passion.

HENRY STEELE COMMAGER  
*Amherst College*

WALTER M. SIMON died unexpectedly at his home in Ashley, Staffordshire, early in June of last year. Professor of history and head of the department at Keele University, he was also prominent in the affairs of that university, serving on many committees as well as being one of three regular advisers of the vice-chancellor.

Although born in Germany, on May 29, 1922, Walter Simon spent his formative years in England. He attended Repton School where, along with a sound education, he acquired his enthusiasm for cricket. His undergraduate training was at Wesleyan University, where he received the B.A. in 1943. From then until 1946 he served in the United States Army. Following the war he studied at Yale under the late Professor

Hajo Holborn; he received his M.A. in 1948 and his Ph.D. in 1949. His first teaching assignment was at Stanford University, where he remained until he moved to Cornell in 1953. In 1965 he accepted the position at Keele.

Although Simon's original field of specialization was modern Germany, his interest early began to shift to intellectual history, particularly to Auguste Comte and the Positivists. His dissertation was published by the Cornell University Press in 1955 under the title *The Failure of the Prussian Reform Movement*, and was followed by articles that eventually were incorporated in his *European Positivism in the Nineteenth Century* (1963). By the time he moved to Keele he seemed to have hit his stride, with volumes appearing every other year: *Germany: A Brief History* (1966), *Germany in the Age of Bismarck* (1968), and *French Liberalism 1789–1848*, which was in the press at the time of his death and is being published by Wiley. In addition, he was at work on a major book on the Enlightenment, had made several contributions to the forthcoming *Dictionary of the History of Ideas*, and was hoping to return to his first love, the French Revolution.

No one who knew him could doubt Walter Simon's dedication to scholarship and teaching, although some, including not a few students, found his standards idiosyncratic as well as exceptionally high. His concept of discipline, intellectual and personal, was exacting, but students willing to work with him on his own terms clearly profited from his influence. As a colleague, in the United States, he was correct, courteous, and reserved—qualities that earned more respect than close friendship, though there were striking exceptions, notably his association with the late Theodor Mommsen.

To many who had known him in the United States he seemed more in his proper element after his return to England where his work flourished and his qualities were at once recognized and appreciated.

E. W. FOX  
*Cornell University*

Other members of the association who have died recently include John Askling of Malaga, New Jersey, and Herbert C. Cohen of C. W. Post College, Greenvale, New York.

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## Other Books Received

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Books listed were received by the AHR between December 1, 1971, and February 1, 1972. Books that will be reviewed are not listed, but listing does not preclude subsequent review.

### GENERAL

APTER, DAVID E., and ANDRAIN, CHARLES F. (eds.). *Contemporary Analytical Theory*. Englewood Cliffs, N. J.: Prentice-Hall. 1972. Pp. x, 688. \$12.50.

BAIROCH, PAUL. *Le Tiers-Monde dans l'impasse: Le démarrage économique du XVIII<sup>e</sup> au XX<sup>e</sup> siècle*. Collection Idées. [Paris:] Gallimard. 1971. Pp. 372.

BAKKE, E. WIGHT, and BAKKE, MARY S. *Campus Challenge: Student Activism in Perspective*. [Hamden, Conn.:] Archon Books. 1971. Pp. x, 573. \$14.50.

BARON, SALO WITTMAYER. *Steeled by Adversity: Essays and Addresses on American Jewish Life*. Ed. by JEANNETTE MEISEL BARON. Philadelphia: Jewish Publication Society of America. 1971. Pp. xi, 729. \$9.00.

BECKFORD, GEORGE L. *Persistent Poverty: Underdevelopment in Plantation Economies of the Third World*. New York: Oxford University Press. 1972. Pp. xxvii, 303. Cloth \$7.50, paper \$2.95.

*Bibliographie internationale de l'Humanisme et de la Renaissance*. Vol. 5, *Travaux parus en 1969*. Fédération internationale des Sociétés et Instituts pour l'étude de la Renaissance. Ouvrage publié sur la recommandation du Conseil International de la Philosophie et des Sciences Humaines, avec le concours du C.N.R.S. et de l'U.N.E.S.C.O. Geneva: Librairie Droz. 1971. Pp. x, 614.

CHERNIAVSKY, MICHAEL, et al. *Social Textures of Western Civilization: The Lower Depths*. In 2 vols. Waltham, Mass.: Xerox College Publishing. 1972. Pp. xvii, 411; xv, 302.

COLLIER, BOYD. *Measurement and Environmental Deterioration*. Research Monograph No. 34. Austin: Bureau of Business Research, University of Texas. 1971. Pp. xi, 100. \$3.00.

CORVISIER, ANDRÉ. *Précis d'histoire moderne*. Paris: Presses Universitaires de France. 1971. Pp. 477. 59 fr.

DECONDE, ALEXANDER (ed.). *Student Activism: Town and Gown in Historical Perspective*. New York:

Charles Scribner's Sons. 1971. Pp. viii, 342. Cloth \$10.00, paper \$3.65.

DIESING, PAUL. *Patterns of Discovery in the Social Sciences*. Observations. Chicago: Aldine-Atherton. 1971. Pp. x, 350. \$11.75.

DOLGOFF, SAM (ed., tr. and with an introd.). *Bakunin on Anarchy: Selected Works by the Activist-Founder of World Anarchism*. Preface by PAUL AVRICH. New York: Alfred A. Knopf. 1972. Pp. xxvii, 405, vii. \$10.00.

GATZKE, HANS W. (ed. with an introd.). *European Diplomacy between Two Wars, 1919-1939*. Modern Scholarship on European History. Chicago: Quadrangle Books. 1972. Pp. 277. \$3.45.

GILBERT, ARTHUR N. (ed.). *In Search of a Meaningful Past*. New Perspectives in History. Boston: Houghton Mifflin. 1972. Pp. x, 233. \$3.95.

GLAZIER, KENNETH M., and HOBSON, JAMES R. *International and English-Language Collections: A Survey of Holdings at the Hoover Institution on War, Revolution and Peace*. Hoover Institution Survey of Holdings: 3. Stanford: Hoover Institution, Stanford University. 1971. Pp. iv, 20. \$2.00.

GOLDMAN, EMMA. *Red Emma Speaks: Selected Writings and Speeches*. Comp. and ed. by ALIX KATES SHULMAN. New York: Random House. 1972. Pp. x, 413. Cloth \$10.00, paper \$2.45.

GRANT, CHARLES. *The War Game*. New York: St. Martin's Press. 1971. Pp. 191. \$8.95.

GREER, THOMAS H. *A Brief History of Western Man*. 2d ed.; New York: Harcourt Brace Jovanovich. 1972. Pp. xii, 546.

GUNST, PÉTER (ed.). *Bibliographia Historiae Rerum Rusticarum Internationalis, 1967-1968*. Budapest: Museum Rerum Rusticarum Hungariae. 1971. Pp. 355.

MARQUIS, ROGER. *English-French, French-English Vocabulary of Prehistoric Archaeology: Vocabulaire français-anglais, anglais-français d'archéologie préhistorique*. Foreword by ALEXIS KLIMOV. Montreal: Presses de l'Université du Québec. 1972. Pp. xii, 42, xii, 43. \$5.00.

MICHEL, ROBERT. *Les partis politiques: Essai sur les tendances oligarchiques des démocraties*. Tr. by S.

JANKELEVITCH. Preface by RENÉ RÉMOND. Science. [Paris:] Flammarion. 1971. Pp. 309.

MICHOFF, NICOLAS V. *Contribution à l'histoire du commerce de la Turquie et de la Bulgarie*. Vol. 6, *Auteurs français, allemands et anglais*. Bulgarska Akademiia na Naukite, Tsentralna Biblioteka. Sofia: Izdatelstvo na Bulgarskata Akademiia na Naukite. 1970. Pp. 573. 5.42 Lv.

MOUSNIER, ROLAND. *Le gerarchie sociali dal 1450 ai nostri giorni*. Ed. by ETTORE ROTELLI. Cultura e storia, No. 7. [Milan:] Editrice Vita e Pensiero. 1971. Pp. 1, 165. L. 3,200.

OECD *Economic Outlook*. No. 10. [Washington:] Organisation for Economic Co-operation and Development. 1971. Pp. 99. \$3.80.

OLSON, RICHARD (ed.). *Science as Metaphor: The Historical Role of Scientific Theories in Forming Western Culture*. Belmont, Calif.: Wadsworth Publishing Co. 1971. Pp. ix, 321.

PETERSON, AGNES F. *Western Europe: A Survey of Holdings at the Hoover Institution on War, Revolution and Peace*. Hoover Institution Survey of Holdings: 1. Stanford: Hoover Institution, Stanford University. 1970. Pp. iv, 60. \$3.00.

REDLICH, FRITZ. *Steeped in Two Cultures: A Selection of Essays*. Torchbook Library Ed. New York: Harper and Row. 1971. Pp. xviii, 430. \$9.00.

ROTELLI, ETTORE, and SCHIERA, PIERANGELO (eds.). *Lo Stato moderno*. Vol. 1, *Dal Medioevo all'età moderna*. Bologna: Società Editrice il Mulino. [1971.] Pp. 294. L. 3,000.

SALPER, ROBERTA (ed., with introds.). *Female Liberation: History and Current Politics*. New York: Alfred A. Knopf. 1972. Pp. xi, 246.

SCHNEIR, MIRIAM (ed. and with an introd. and commentaries). *Feminism: The Essential Historical Writings*. New York: Random House. 1972. Pp. xxi, 360. Cloth \$7.95, paper \$2.45.

STAMMER, OTTO (ed.). *Max Weber and Sociology Today*. Tr. by KATHLEEN MORRIS. Explorations in Interpretative Sociology. Torch Book Library Ed. New York: Harper and Row. 1971. Pp. viii, 256. \$7.50.

STEARNS, PETER N. (ed.). *The Impact of the Industrial Revolution: Protest and Alienation*. Englewood Cliffs, N. J.: Prentice-Hall. 1972. Pp. vi, 186. Cloth \$5.95, paper \$2.45.

STERN, B. E., and LEWIS, D. *X-Rays*. [London:] Pitman Publishing; distrib. by Harper and Row, New York. 1971. Pp. viii, 305. \$11.00.

SUSSMAN, IRVING. *As Others See Us: A Look at the Rabbi, Priest and Minister through the Eyes of Literature*. New York: Sheed and Ward. 1971. Pp. vi, 266. \$7.50.

SUTMAN, FRANCIS X. (ed.). *What Kind of Environment Will Our Children Have?* Proceedings of the AACTE/OAS Conference on Education and the Environment in the Americas. Sponsored by American Association of Colleges for Teacher Education and the Organization of American States. Held in Washington, D. C. October 19-22, 1970. [Washing-

ton:] American Association of Colleges for Teacher Education. 1971. Pp. x, 92.

TELFORD, SHIRLEY. *Economic and Political Peace*. 2d ed.; Portland, Ore.: William and Richards. 1972. Pp. xiv, 236. \$5.95.

TOMPKINS, E. BERKELEY (ed. with an introd.). *Peaceful Change in Modern Society*. Hoover Institutions Publications 101. Stanford: Hoover Institution Press, Stanford University. 1971. Pp. xi, 158. Cloth \$6.50, paper \$3.95.

Trotsky, Leon, *Writings of (1934-35)*. New York: Pathfinder Press. 1971. Pp. 364. Cloth \$8.95, paper \$3.45.

WOODWARD, HERBERT N. *The Human Dilemma*. New York: Brookdale Press. 1971. Pp. x, 198. \$5.95.

## ANCIENT

HANSEN, ESTHER V. *The Attalids of Pergamon*. Cornell Studies in Classical Philology, Vol. 36. 2d rev. ed.; Ithaca: Cornell University Press. 1971. Pp. xx, 531. \$27.50. See rev. of 1st ed. (1947), *AHR*, 53 (1947-48): 526.

HERBERT, KEVIN. *Greek and Latin Inscriptions in the Brooklyn Museum*. Brooklyn: Brooklyn Museum. 1972. Pp. xvii, 93, 28 plates. \$8.00.

PINGREE, DAVID. *Census of the Exact Sciences in Sanskrit*. Ser. A, Vol. 2. Memoirs of the American Philosophical Society, Vol. 86. Philadelphia: the Society. 1971. Pp. vii, 147. \$6.00.

## MEDIEVAL

BACHRACH, BERNARD S. (ed.). *The Medieval Church: Success or Failure?* European Problem Studies. New York: Holt, Rinehart and Winston. 1972. Pp. 120.

DOMMANGET, MAURICE. *La Jacquerie*. Petite Collection Maspero 95. Paris: François Maspero. 1971. Pp. 125.

ERGANG, ROBERT. *Emergence of the National State*. Anvil Original. New York: Van Nostrand Reinhold. 1971. Pp. vi, 249. \$3.50.

FÉDOU, RENÉ. *L'État au Moyen Âge*. Collection SUP, "L'historien," No. 8. Paris: Presses Universitaires de France. 1971. Pp. 212.

HUNT, NOREEN (ed.). *Cluniac Monasticism in the Central Middle Ages*. [Hamden, Conn.:] Archon Books. 1971. Pp. x, 248. \$15.00.

JOHN OF PARIS. *On Royal and Papal Power*. Tr. with an introd. by J. A. WATT. Toronto: Pontifical Institute of Mediaeval Studies. 1971. Pp. 261.

## BRITISH COMMONWEALTH AND IRELAND

CAMERON, WILLIAM J. (ed.). *Poems on Affairs of State: Augustan Satirical Verse, 1660-1714*. Vol. 5, 1688-1697. New Haven: Yale University Press. 1971. Pp. xliii, 650. \$25.00.

Canada. OECD Economic Surveys. [Washington:] Organisation for Economic Co-operation and Development. 1971. Pp. 65. \$1.00.

GARDINER, LESLIE. *The Royal Oaks Courts Martial*. Annapolis: United States Naval Institute. 1965. Pp. xi, 258. \$4.75.

GRAYSON, L. M., and BLISS, MICHAEL (eds., with an introd.). *The Wretched of Canada: Letters to R. B. Bennett 1930-1935*. The Social History of Canada. [Toronto:] University of Toronto Press. 1971. Pp. xxvii, 199. Cloth \$12.50, paper \$3.95.

HIGHAM, ROBIN (ed.). *A Guide to the Sources of British Military History*. Sponsored by the Conference on British Studies. Berkeley and Los Angeles: University of California Press. 1971. Pp. xxi, 630. \$22.50.

HUSSEY, W. D. *British History, 1815-1939*. New York: Cambridge University Press. 1971. Pp. 367. \$6.95.

*List of Gifts and Deposits in the Scottish Record Office*. Vol. 1. Edinburgh: H. M. Stationery Office. 1971. Pp. vi, 123. £2.75.

MOODY, T. W. (ed.). *Irish Historiography, 1936-70*. Dublin: Irish Committee of Historical Sciences. 1971. Pp. viii, 155. £1.00.

NEW, JOHN F. H. (ed.). *Oliver Cromwell: Pretender, Puritan, Statesman, Paradox?* European Problem Studies. New York: Holt, Rinehart and Winston. 1972. Pp. 124.

PUBLIC ARCHIVES OF CANADA. *Report, 1959-1969*. [Ottawa: the Archives.] 1971. Pp. 92, 96.

SCHULTZ, HAROLD J. (ed. and with an introd.). *English Liberalism and the State: Individualism or Collectivism? Problems in European Civilization*. Lexington, Mass.: D. C. Heath. 1972. Pp. xxiv, 152.

SEELEY, J. R. *The Expansion of England*. Ed. and with an introd. by JOHN GROSS. Classics of British Historical Literature. Chicago: University of Chicago Press. 1971. Pp. xxvii, 248. \$9.50.

*United Kingdom*. OECD Economic Surveys. [Washington:] Organisation for Economic Co-operation and Development. 1971. Pp. 43. \$1.00.

WALVIN, JAMES. *The Black Presence: A Documentary History of the Negro in England, 1555-1860*. Sourcebooks in Negro History. New York: Schocken Books. 1972. Pp. 222. \$8.50.

ZIMANSKY, CURT A. (ed.). *The Eighteenth Century: A Current Bibliography for 1970*. Philological Quarterly, Vol. 50, No. 3. Iowa City: University of Iowa. 1971. Pp. 321-531. Cloth \$6.50, paper \$4.50.

#### FRANCE

FRAGHER, RICHARD. *Life and Letters in France: The Eighteenth Century*. New York: Charles Scribner's Sons. [1972.] Pp. xxvi, 235. Cloth \$7.95, paper \$3.95.

HOWARTH, W. D. *Life and Letters in France: The Seventeenth Century*. New York: Charles Scribner's Sons. [1972.] Pp. xxvi, 237. Cloth \$7.95, paper \$3.95.

KNAPTON, ERNEST JOHN. *Revolutionary and Imperial France, 1750-1815*. New York: Charles Scribner's Sons. 1972. Pp. xi, 147. \$6.95.

MOUSNIER, ROLAND. *La vénalité des offices sous Henri*

*IV et Louis XIII*. "Collection Hier." 2d rev. ed.; Paris: Presses Universitaires de France. 1971. Pp. 724. 95 fr. See rev. of 1st ed. (1946), *AHR*, 52 (1946-47): 113.

RAITT, A. W. *Life and Letters in France: The Nineteenth Century*. New York: Charles Scribner's Sons. [1972.] Pp. xxx, 177. Cloth \$7.95, paper \$3.95.

WEISS, LOUISE. *Le sacrifice du chevalier, 3 septembre 1939-9 juin 1940*. Mémoires d'une Européenne, new ser. Paris: Albin Michel. 1971. Pp. 318. 24 fr.

#### GERMANY, AUSTRIA, AND SWITZERLAND

ADAMS, MARION (ed.). *The German Tradition: Aspects of Art and Thought in the German-Speaking Countries*. New York: John Wiley and Sons. 1971. Pp. xi, 220. \$5.95.

BARKER, THOMAS M. (ed.). *Frederick the Great and the Making of Prussia*. European Problem Studies. New York: Holt, Rinehart and Winston. 1972. Pp. 103.

RÖHL, JOHN C. G. *Zwei deutsche Fürsten zur Kriegsschuldfrage: Lichnowsky und Eulenburg und der Ausbruch des Ersten Weltkriegs. Eine Dokumentation*. Düsseldorf: Droste Verlag. 1971. Pp. 80. DM 14.

ROTH, ERNST. *A Tale of Three Cities*. New York: Charles Scribner's Sons. 1971. Pp. ix, 178. \$6.95.

STERN, FRITZ. *The Failure of Illiberalism: Essays on the Political Culture of Modern Germany*. New York: Alfred A. Knopf. 1972. Pp. xlv, 233, xii. \$7.95.

#### ITALY

BESUTTI, GIUSEPPE M., O.S.M. *Bibliografia dell'Ordine dei Servi*. Vol. 1, *Repertori e sussidi generali; Edizioni del secolo XV (1476-1500)*. Aristide M. Serra O.S.M. *Memoria di Fra Paolo Attavanti*. Bibliotheca Servorum Romandiola, No. 4. Bologna: Centro di Studi O.S.M. 1971. Pp. 266.

CAMPANELLA, ANTHONY P. (comp. with introd. and notes). *Giuseppe Garibaldi e la tradizione garibaldina: Una bibliografia dal 1807 al 1970*. In 2 vols. Geneva: Comitato dell'Istituto Internazionale di Studi Garibaldini. 1971. Pp. xxvii, 595; vi, 600-1311. \$22.00 the set.

CARLO PISACANE and GIUSEPPE LA MASA. *La guerra del 1848-49 in Italia*. Ed. by SALVATORE SECHI. *Classici della cultura italiana*. Section: Storica. Naples: Casa Editrice Fulvio Rossi. 1969. Pp. 593. L. 7,000.

GUNN, PETER. *A Concise History of Italy*. Studio Book. New York: Viking Press. 1971. Pp. 224. \$10.00.

#### EASTERN EUROPE

FISCHER-GALAJI, STEPHEN (ed.). *Man, State, and Society in East European History*. Man, State, and Society. New York: Praeger. 1970. Pp. xiii, 343. Cloth \$11.00, paper \$4.95.

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## SPAIN AND PORTUGAL

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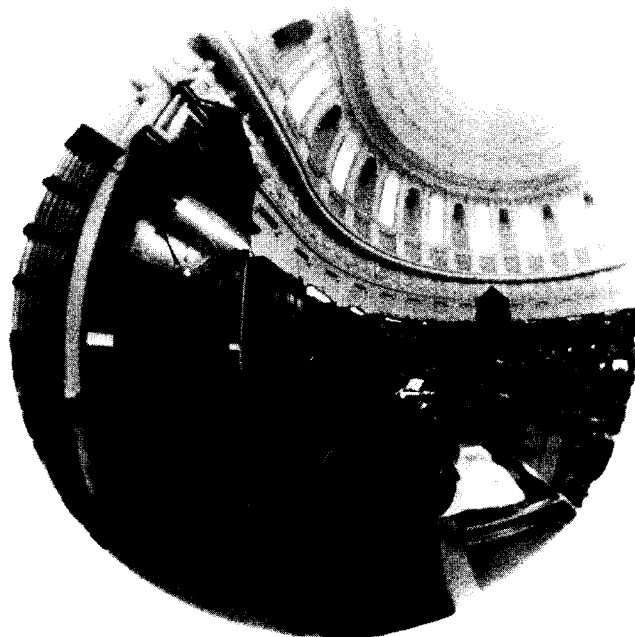
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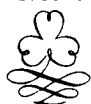
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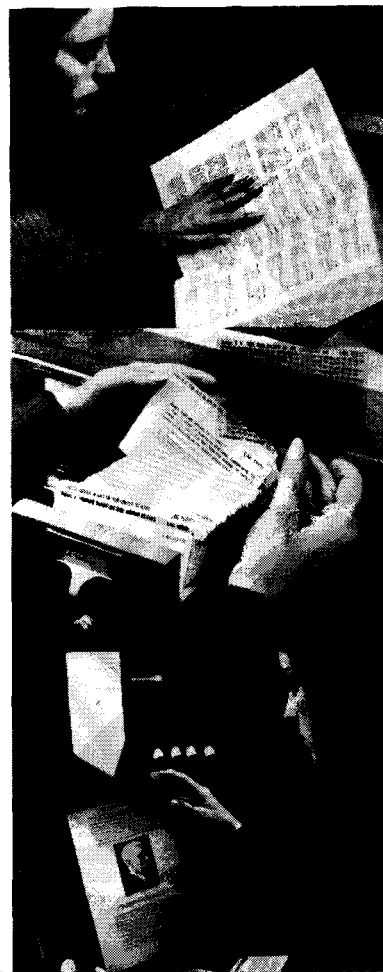
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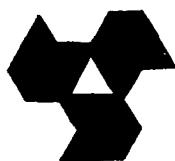
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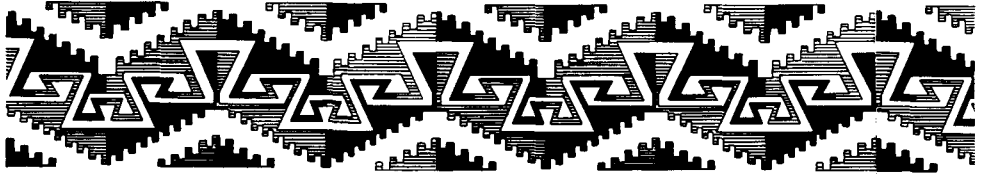
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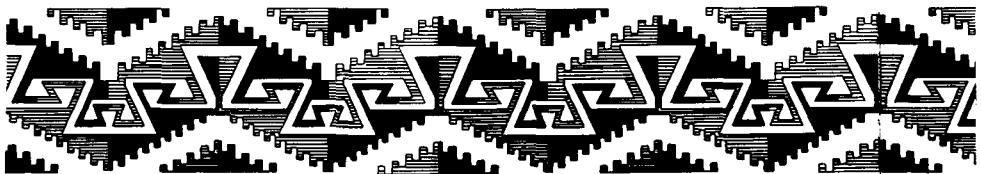
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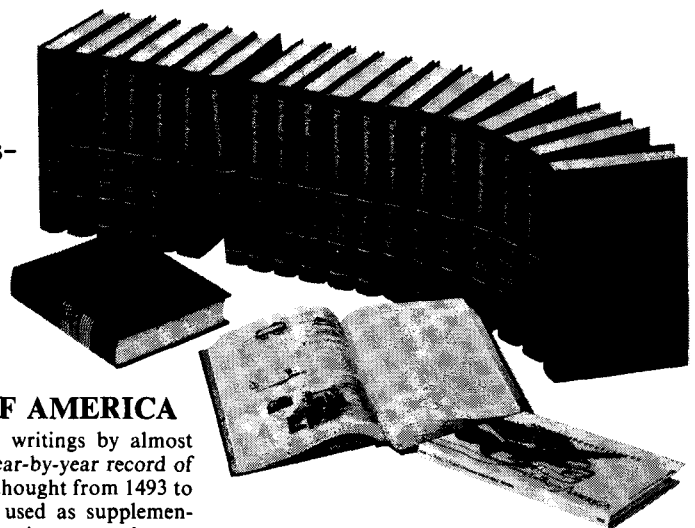
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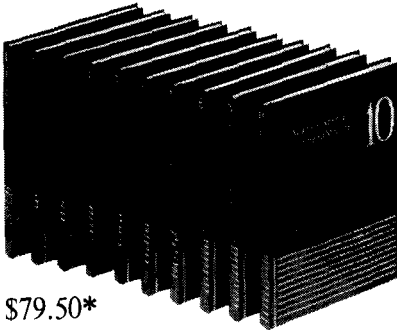
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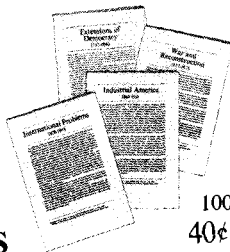
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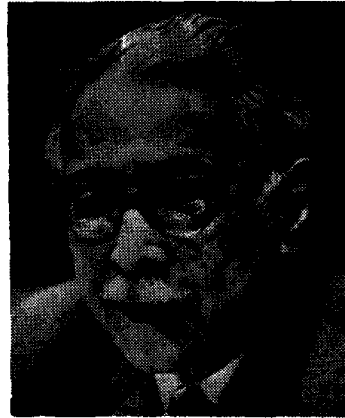


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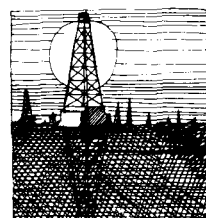
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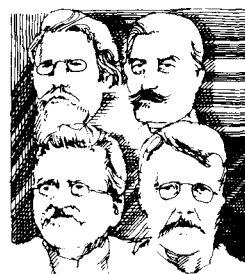
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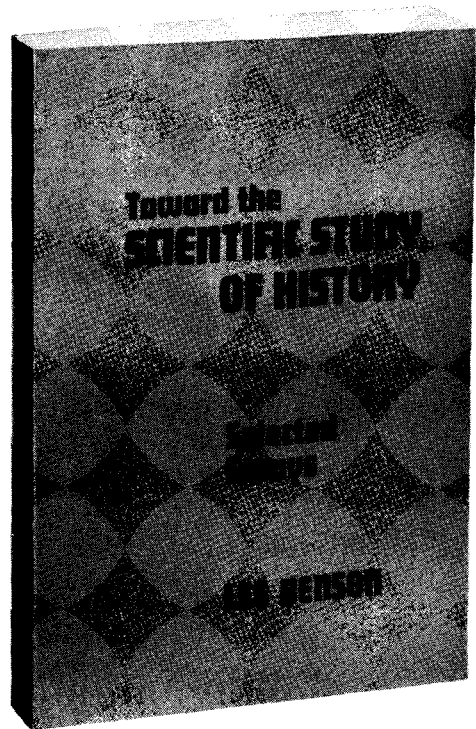
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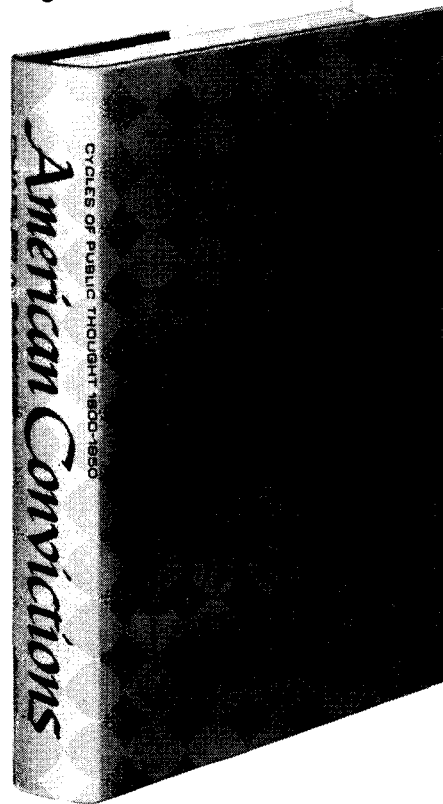
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